THE PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND COMMERCE.

By ALEXANDER TILLOCH, LL.D.

M.R.I.A. F.S.A. EDIN. AND PERTH; CORRESPONDING MEMBER OF THE ROYAL ACADEMY OF SCIENCES, MUNICH; AND OF THE ACADEMY OF SCIENCES, LITERATURE AND ARTS, LEGHORN, ETC.

And RICHARD TAYLOR, F.L.S.

MEMBER OF THE ASTRONOMICAL SOCIETY OF LONDON, OF THE METEOROLOGICAL SOCIETY; AND OF THE ROYAL ASIATIC SOCIETY OF GREAT BRITAIN AND IRELAND.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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THE

PHILOSOPHICAL MAGAZINE
AND JOURNAL.

31st JANUARY 1825.

I. *On the Method of the Least Squares.* By J. Ivory, *Esq. M.A. F.R.S.*

THE present advanced state of astronomy is to be ascribed chiefly to the precision of the theories, and to the great exactness of modern observations. But there is another circumstance which has likewise contributed much to the same end; and that is, the method of combining the observations so as to draw from them the most advantageous results. In general a very few observations are sufficient for approximating to the quantity of an astronomical element with a considerable degree of precision. But the errors unavoidable by the most skilful observers must always leave some uncertainty in such determinations. The utmost precision can only be obtained by multiplying the observations, and by making a great number, embracing every possible variety of circumstances, concur in the valuation of the same element. Astronomers accomplish this end by the use of *equations of condition.*

Let V denote an astronomical function, varying in different circumstances, but involving one, or several determinate elements. Suppose that the values of the elements have been found nearly; and let the corrections, or the differences between the approximate and the exact values, be denoted by the small quantities x, y, z, &c.: then if, for every element, we substitute the sum of the approximate value and the correction, and expand into a series, neglecting the powers and products of the small quantities x, y, z, &c., we shall obtain,

$$V = V' + ax + by + cz + \&c.$$

Let O be an observed value of V: if O were rigorously exact, we should have $O = V$; but as every observation is liable to error, if we denote the error by e, we shall have

$O + e = V$. Wherefore, by substituting the development of V, and putting $m = O - V'$, we shall obtain this *equation of condition*,

$$e = -m + ax + by + cz + \&c.$$

Every observation will furnish a like equation. In this problem, therefore, we have a system consisting of any number of equations, viz.

$$\begin{aligned} e &= -m + ax + by + cz + \&c. \\ e' &= -m' + a'x + b'y + c'z + \&c. \\ e'' &= -m'' + a''x + b''y + c''z + \&c. \\ &\&c. \end{aligned}$$

And it is required to determine the corrections x, y, z, so as to make the errors e, e', e'', &c. either absolutely equal to zero when this is possible, otherwise so that they shall be contained within the least limits. When the number of the equations is just equal to the unknown quantities x, y, z, &c., the errors being supposed evanescent, the problem will come under the usual rules of algebra, and will admit of an exact solution. But in the cases that occur in practice, the number of equations being greater than the corrections to be found, no mode of solution will entirely annihilate the errors, and we must be content with reducing them to the least possible quantities.

Cotes appears to have introduced the use of equations of condition. In the most simple case of only one element, the system of equations is as follows, viz.

$$\begin{aligned} e &= ax - m \\ e' &= a'x - m' \\ e'' &= a''x - m'' \\ &\&c. \end{aligned}$$

Supposing every error equal to zero, Cotes finds the several values of x, viz. $\frac{m}{a}$, $\frac{m'}{a'}$, $\frac{m''}{a''}$, &c.; and, by applying these corrections, as many determinations of the element are obtained as there are observations. Now, let all the values thus found be set off, on the same side, from a given point in a straight line; and let the weights a, a', a'', &c. be appended to the extremities of the several parts; then the distance of the centre of gravity of all the weights from the given point will, according to Cotes, be the most advantageous value of the element. By this process the correction to be added to the approximate element comes out equal to

$$\frac{m + m' + m'' + \&c.}{a + a' + a'' + \&c.}.$$

We shall obtain the result of Cotes's method more simply by adding all the equations of condition, and making the sum of

of the errors equal to zero; for the value of x found in this manner will coincide with the expression just set down. And hence we may conclude, that the mode of combining the equations of condition imagined by Cotes is not the most advantageous. For, as the total sum of the errors is equal to zero, an error of a given amount in any of the equations has the same influence on the value of the correction. But it is very evident that the error e produces a small variation in the value of x when a is a great number; and, on the contrary, a great variation when the same coefficient is inconsiderable. The procedure of Cotes is therefore just and unexceptionable only when all the coefficients, a, a', a'', &c. are equal, or when all the errors are in like circumstances.

Reflecting on what has just been said, we may, by a slight alteration in the procedure of Cotes, deduce a better way of combining the equations, and one which is more advantageous than any other. The influence of the error e on the value of the correction x is less when the coefficient a is greater, and it increases when the same coefficient is diminished in magnitude. This is exactly similar to a lever which is to produce a given effect; for the lever must be shortened when the suspended weight is greater, and lengthened when the same weight is less. Draw a straight line, and from a given point in it set off all the positive errors on one side, and the negative errors on the other side; then, having suspended the weights a, a', a'', &c. from the levers e, e', e'', &c., make the levers in equilibrio about the common fulcrum. By this construction the errors will be so determined that the coefficients a, a', a'', &c. will have each its proper influence on the value of the correction x. The equation of the equilibrium is

$$e\,a + e'\,a' + e''\,a'' + \&c. = 0; \qquad \text{(A)*}$$

and, if we observe that x is the only variable quantity in the expression of the errors, the same equation may be thus written, viz.

$$e\,\frac{de}{dx} + e'\,\frac{de'}{dx} + e''\,\frac{de''}{dx} + \&c. = 0,$$

which determines the minimum of the function

$$e^2 + e'^2 + e''^2 + \&c.$$

* It is evident that here, as well as in what follows, we speak only of irregular and fortuitous errors that have no constant part. If we suppose a part common to all the errors, there would not be an equilibrium of the levers, but a preponderance. Let $\pm f$ denote the constant error; then, in place of equat. (A), we should have this which follows, viz.

$$a\,e + a'\,e' + a''\,e'' + \&c. = \mp (a + a' + a'' + \&c.) \times f;$$

and f would be the distance between the common fulcrum of all the levers and the centre of gravity of the weights a, a', a'', &c.

Thus

Thus the condition of the equilibrium of the levers makes the sum of the squares of the errors a minimum; and hence we may infer that the errors themselves are contained within the least limits on either side of the common fulcrum. If the errors vary from the minimum, they cannot all decrease; if any of them decrease, others must increase; and some of them at least must be greater than in the case of the minimum. There appears, therefore, to be sufficient reason for preferring the minimum of the squares of the errors as the most advantageous solution of a system of equations of condition.

Let us now place the matter in a different light. In any system of errors of observation, supposing that there is no constant cause of deviating from the truth, the total sum will be equal to zero. At least, this will be the case if the observations be numerous, and if they embrace every possible variety of circumstances. When a cause exists tending either equally to augment, or equally to diminish, all the observations, the sum of the errors divided by their number will determine the constant quantity affecting every observation. It is evident that these considerations are independent of the magnitude of the errors; and therefore they can be of no use in solving a system of equations of condition, where the object is to make all the errors fall within the least possible limits. Let us next consider the sum of the squares of a system of errors, viz.

$$e^2 + e'^2 + e''^2 + \&c.$$

This sum is augmented by every observation, and it will therefore increase indefinitely with their number. But as every error lies between zero and a certain limit, if the sum of their squares be divided by their number, the quotient, or the mean of the squares, will also be contained between zero and a limit; and it will approach more nearly to a determinate value as the observations are more numerous. Therefore, in a system of observations made in like circumstances, the mean of the squares of the errors will be a quantity independent of their number, varying in its magnitude only as the errors are more or less considerable, and affording with some accuracy a measure of the precision of the observations. In several sets of observations made for the same purpose by different observers and in different circumstances, that one in which the mean of the squares of the errors is least must be considered as possessed of the greatest degree of precision, and would deserve the preference. Now, in solving a system of equations of condition in several different ways, the errors will acquire different magnitudes, just as happens in several sets of observations of unequal degrees of precision. We will,

will, therefore, fix upon the best mode of solution by the same rule that we employ for ascertaining the most advantageous of several sets of observations. That mode of solution is, therefore, to be preferred, in which the mean of the squares of the errors is the least. But, in all possible ways of solving a system of equations of condition, the number of the errors is constantly the same; and therefore the mean quantities are proportional to the total sums. And hence we must conclude, as we have already found by a different train of reasoning, that the most advantageous result will be obtained when the equations are combined so as to render the sum of the squares of the errors a minimum.

It must, however, be allowed that every thing which has just been said of the sum of the squares of the errors, and of the mean of the squares, will equally apply to the sums and the mean quantities of any of their even powers. Thus the latter demonstration leaves it doubtful whether it is the sum of the squares of the errors, or the sum of any other of their even powers, that must be a minimum in order to obtain the most advantageous result. But it is easy to exclude the other even powers, and to render the demonstration absolute with regard to the squares. The equation of the minimum of the function,

$$e^{2n} + e'^{2n} + e''^{2n} + \&c.$$

is this,

$$\frac{de}{dr} e^{2n-1} + \frac{de'}{dx} e'^{2n-1} + \frac{de''}{dx} e''^{2n-1} + \&c. = 0,$$

or,

$$a e^{2n-1} + a' e'^{2n-1} + a' e''^{2n-1} + \&c. = 0:$$

and as this is true in all relations of a, a', a'', &c., it will be true when all these coefficients are equal; in which case the equation will become,

$$e^{2n-1} + e'^{2n-1} + e''^{2n-1} + \&c. = 0.$$

But the supposition of the equality of a, a', a'', &c. places all the errors in like circumstances; and it is universally admitted that in a series of observations made in like circumstances, the simple sum of the errors is equal to zero, and not the sum of their cubes, or fifth powers, or any other of their odd powers. By this argument the demonstration is restricted to the squares of the errors, and the other even powers are excluded.

The first of the two demonstrations founded on the principle suggested by Cotes is clear and simple, and leads exclusively to the method of the least squares. It is probably the best and most solid demonstration that can be given. The

The equation (A) must be considered as a generalization, drawn from the nature of the equations of condition, of the rule universally admitted by astronomers, that the sum of the errors of a numerous set of observations made in like circumstances is equal to zero; which rule is itself contained in the equation, being that particular case of it when the coefficients are all equal. But although the first demonstration be sufficient to establish the proposition, yet the second, as it throws additional light on the matter, may not be deemed superfluous.

It is easy to apply to two or more elements what has been proved with respect to one. In the case of two elements we have this system of equations, viz.

$$\begin{aligned} e &= a\,x + b\,y - m \\ e' &= a'x + b'y - m' \\ e'' &= a''x + b''y - m'' \\ &\text{\&c.} \end{aligned}$$

The corrections x and y being independent of one another, the errors must be determined so as to give their proper influence both to the coefficients a, a', a'', &c. and to the coefficients b, b', b'', &c. The levers e, e', e'', &c. must therefore be in equilibrio when the weights a, a', a'', &c. are suspended; and again, when these are removed, and the other weights b, b', b'', &c. are substituted for them. These two states of equilibrium lead to the equations

$$\begin{aligned} a\,e + a'e' + a''e'' + \text{\&c.} &= 0 \\ b\,e + b'e' + b''e'' + \text{\&c.} &= 0, \end{aligned}$$

which are sufficient for finding both the unknown quantities x and y. The same two equations may be otherwise written thus, viz.

$$\frac{de}{dx}\,e + \frac{de'}{dx}\,e' + \frac{de''}{dx}\,e'' + \text{\&c.} = 0$$

$$\frac{de}{dy}\,e + \frac{de'}{dy}\,e'' + \frac{de''}{dy}\,e'' + \text{\&c.} = 0,$$

of which the first determines the minimum of the function,

$$e^2 + e'^2 + e''^2 + \text{\&c.}$$

relatively to the variable x; and the other determines the same minimum relatively to the variable y. Both the equations together determine the absolute minimum of the function relatively to both the variables; which condition therefore contains the full solution of the problem.

In the case of three corrections, viz.

$$\begin{aligned} e &= a\,x + b\,y + c\,z - m \\ e' &= a'x + b'y + c'z - m' \\ e'' &= a''x + b''y + c''z - m'' \\ &\text{\&c.} \end{aligned}$$

we

we have three sets of weights, viz. a, a', a'', &c., and b, b, b'', &c., and c, c', c'', &c.; and the levers e, e', e'', &c. must be in equilibrio when each set separately is suspended from their extremities. The equations necessary for this purpose are these three, viz.

$$a e + a' e' + a'' e'' + \&c. = 0$$
$$b e + b' e' + b'' e'' + \&c. = 0$$
$$c e + c' e' + c'' e'' + \&c. = 0,$$

which are sufficient for finding the corrections x, y, z. The same equations may be thus written, viz.

$$\frac{de}{dx} e + \frac{de'}{dx} e' + \frac{de''}{dx} e'' + \&c. = 0$$
$$\frac{de}{dy} e + \frac{de'}{dy} e' + \frac{de''}{dy} e'' + \&c. = 0$$
$$\frac{de}{dz} e + \frac{de'}{dz} e' + \frac{de''}{dz} e'' + \&c. = 0,$$

and these determine the minima of the sum of the squares of the errors relatively to the variables x, y, z respectively. The same three equations determine the absolute minimum of the function relatively to all the variables; and therefore, in this single condition the full solution of the problem is contained. It is evident that the same mode of reasoning will apply to the simultaneous correction of any number of elements by means of a system of equations of condition.

The procedure which we have demonstrated has very properly been called the *method of the least squares*, since the absolute minimum of the sum of the squares of the errors contains all the conditions necessary for finding the several corrections. It was first published by M. Legendre in his treatise on the Orbits of Comets. But some years before the publication of that work, M. Gauss, of Göttingen, had likewise found out the same process, which he had communicated to his astronomical friends; and he was in the habit of applying it in astronomical researches.

We have here attempted to demonstrate the method of the least squares, from the nature of the equations of condition, and from the principle that, in the most advantageous solution, the errors of the equations must be contained within the least limits. But it has been usual to introduce the doctrine of probabilities in order to explain this theory. A little attention will show that all such investigations are founded on arbitrary suppositions. We cannot compute the probability of any combination of the errors, without first assuming a general function for expressing the probability of an error taken indefinitely. There are many properties which such

a function must possess; but it is impossible that one can be found that will accurately represent a probability, of which we never can acquire a perfect knowledge. Therefore, although the same conclusion be still brought out, yet, as the reasoning involves precarious suppositions, the fundamental principles will not be placed in a light so simple and clear and steady as when nothing extraneous is introduced in the demonstration. It has been proved in the case of a great number of observations, that the principle of the least squares will hold good, whatever law of probability be adopted; and hence we may derive an argument that such laws are foreign to the demonstration, since they disappear at the conclusion. If we admit that, whatever be the expression of the chance of an error, the mean of the squares of the errors will converge to a fixed limit as the observations increase in number, the second of the two demonstrations given above will be conclusive in all possible laws of probability.

But, allowing that the method of the least squares is fully established, it is not sufficient for the purposes of astronomy in its present state of improvement. An element, although computed in the best way, can only have a degree of accuracy relative to the precision of the observations. It thus becomes necessary to estimate the uncertainty in the result obtained, or to assign the probability that the remaining error amounts to a tenth, or some given part, of the whole. But the discussion of the subject in this new view of it would exceed our present limits, and must be left to a future occasion.

January 8, 1825. JAMES IVORY.

II. *Hints tending to disprove the Existence of distinct Calorific Rays in the Sunbeam.* By Mr. HENRY MEIKLE.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THAT great mechanical genius Dr. Hooke seems to have been the first who suspected that the sunbeam contains rays which illuminate without producing heat. In this opinion he has been followed successively by M. Scheele, Sir William Herschel, and Sir Henry Englefield, who are well known as able experimenters. Among other conclusions, the two last thought they had established that the sun emits several distinct sorts of rays, especially illuminating rays, as already mentioned, which give no heat; and on the other hand calorific rays which are not accompanied with light. For on placing a thermometer in the well-known figure called the spectrum,

spectrum, this thermometer seemed to be more affected the nearer it was placed to the red margin, and less as it approached the opposite or violet-coloured edge. But the most remarkable effect of all was, that the thermometer indicated the greatest heat when placed just without the red margin, where none of the visible rays reached it at all. They therefore inferred that this effect was produced by a set of dark calorific rays which are less refrangible than any of the other rays. On repeating these experiments, M. Berard obtained similar results, except that he found the maximum of heat in the red ray.

But notwithstanding the high reputation of these philosophers, their conclusion has been questioned by Professor Leslie of Edinburgh, who by experimenting somewhat differently was unable to detach any of these dark rays from the light. Having rendered a circular spot opaque in the middle of a large convex lens, he received the light transmitted by the remaining transparent ring upon a surface of black wax, held at such a distance that the light formed upon the wax an iris, or ring, composed of a set of distinct concentric rings, which severally possessed all the various colours of the common spectrum. Mr. Leslie then carefully observed the effect of these rings on the wax, and found that none of it was melted beyond where it was covered by the iris; whereas if a set of dark calorific rays had existed, these ought to have more thoroughly melted a larger ring than that whereon the light fell; for the dark rays, if less refrangible than light, would have fallen without the margin of the red ring, which includes all the others.

Now, since this experiment of Professor Leslie is of a more simple and decisive cast than any performed by the gentlemen already mentioned, I am much inclined to give it the preference, and to conclude with him that the rays of light only produce or become heat when they themselves disappear. It is also well known that the more perfectly any surface reflects light, the less will that surface itself be heated by the light.

As I do not recollect to have seen any reason given why Sir William Herschel's mode of experimenting ought to have apparently produced the effect he observed, without calling in the aid of dark calorific rays, I shall take the liberty of briefly suggesting what I suspect to have been the principal source of deception. If a prism such as Sir William employed be heated, a very delicate thermometer will, *cæteris paribus*, be more affected when it is held opposite to one of the flat sides of the prism than when opposite to one of its edges;

 because

because heat escapes from glass and many other substances, when smooth or polished, chiefly in straight lines perpendicular to the surface. Now if we attend to the position of Sir William Herschel's prism and thermometer *, this will help to explain why the thermometer indicated heat, even when none of the illuminating rays reached it at all; as also why the heating power of the red rays seemed so much to surpass that of the other colours, &c.: because the more directly opposite the thermometer was to the flat side of the prism, the more of its heat would it receive; and in the course of such an experiment, there can be little doubt that the prism became considerably heated by absorbing a portion of the solar rays.

The circumstance of heat emerging from an even or polished surface chiefly in perpendicular lines will help to explain various phænomena. From it we learn that Mr. Leslie's lens, however hot, could have little or no tendency to melt the wax; because the heat would emanate from its convex surface chiefly in a set of diverging straight lines, such as might be drawn from the centre of the sphere of which the lens formed a part.

For aught that is known to the contrary, the heating powers of all the rays in the spectrum may be equal; because the seeming difference may be owing to a difference of density, some of them being probably much dilated or attenuated by refraction. I am, gentlemen, yours &c.

Dec. 21, 1824. HENRY MEIKLE.

III. *On the Experiments of the Pendulum made by Captain* KATER, *M.* BIOT, *&c. By* WM. GALBRAITH, *Esq. A.M.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN my paper published in your Magazine for September 1824, I gave, page 163, a general formula (7) to determine the ellipticity of the meridian from observations on the length of the pendulum made in different latitudes. In this formula, however, it is necessary to substitute quantities which, with the exception perhaps of the time of rotation, are not yet sufficiently well determined. The accuracy of these is constantly improving, and in a short time we expect to have the length of the pendulum oscillating seconds on the equator ascertained by different observers in several points of that circle

* Diagrams or plates illustrative of this may be seen in so many books, that it would be superfluous to introduce them here.

with

with great precision. The radius of the equator, though not perfectly exact, may be depended upon as being known to within a few hundred feet of the truth. By making use of those of the best authority we arrived at formula (9), page 164,

or, $$e = \tfrac{5}{2} \times \frac{20919576}{20919576 + 1856062635\,z} - \frac{y}{z}: \qquad (1)$$

in which z, the length of the seconds pendulum at the equator, and y, the excess of the seconds pendulum at the pole above that at the equator, are the only unknown quantities.

As the equator is accessible in many points, we may shortly expect to have z well determined; though it will not be easy, perhaps impossible, ever to obtain y by actual experiment.

We have used this care in detailing the manner in which we obtained the general formula, and selected the values of the quantities employed, that it might be apparent what confidence results derived from it deserved. This was the more necessary, as we have shown, page 169, that the ellipticity derived from the measurement of arcs, and that determined from the observations by the pendulum, were in mean latitudes, as well as extreme latitudes, though in a less degree, directly opposed to each other, upon the assumption that the arc of the meridian is a portion of a regular ellipse*.

As we discussed the experiments of Kater, Biot, &c., separately in our last communication, we have, in order to throw some light, if possible, upon this anomaly, here combined them along with others which we have since obtained, making as nearly as possible a regular series of them from the equator towards the north pole, retaining one only, that at Rio Janeiro, in the southern hemisphere, as being near the equator, and having without it, proportionally to the others, rather too few near that circle.

Pursuing the same method as formerly, we obtain

1. $39{\cdot}01951 - z - 0{\cdot}0000884\,y = e_1$
2. $39{\cdot}02339 - z - 0{\cdot}0511335\,y = e_2$
3. $39{\cdot}04115 - z - 0{\cdot}1347996\,y = e_3$
4. $39{\cdot}04602 - z - 0{\cdot}1517026\,y = e_4$
5. $39{\cdot}09419 - z - 0{\cdot}3903417\,y = e_5$
6. $39{\cdot}11319 - z - 0{\cdot}4932370\,y = e_6$
7. $39{\cdot}11300 - z - 0{\cdot}4972172\,y = e_7$
8. $39{\cdot}11809 - z - 0{\cdot}5136117\,y = e_8$
9. $39{\cdot}12928 - z - 0{\cdot}5667720\,y = e_9$
10. $39{\cdot}13770 - z - 0{\cdot}6045723\,y = e_{10}$
11. $39{\cdot}13929 - z - 0{\cdot}6127966\,y = e_{11}$

* At page 169, line 25, of my last communication, between $\frac{1}{310}$ and *not*, the words "*gives an ellipticity*" should have been inserted.

12.

$$
\begin{array}{ll}
12. & 39{\cdot}14250 - z - 0{\cdot}6246030\,y = e_{12} \\
13. & 39{\cdot}14600 - z - 0{\cdot}6455596\,y = e_{13} \\
14. & 39{\cdot}15549 - z - 0{\cdot}6869391\,y = e_{14} \\
15. & 39{\cdot}16159 - z - 0{\cdot}7142003\,y = e_{15} \\
16. & 39{\cdot}17162 - z - 0{\cdot}7613587\,y = e_{16} \\
17. & 39{\cdot}19840 - z - 0{\cdot}8878916\,y = e_{17} \\
18. & 39{\cdot}20700 - z - 0{\cdot}9311390\,y = e_{18}
\end{array}
$$

Taking the sums of the respective quantities, and dividing by their number,

$$z = 39{\cdot}1193006 - 0{\cdot}5148869\,y \qquad (2)$$

Where Kater and Biot observed at the same place, we have taken the mean between them, reduced to the same temperature, namely 62° of Fahrenheit. The other experiments combined with them are, from the data of the observers, together with the known expansion of the brass of which the pendulum was constructed, corrected in like manner.

Again,

$$
\begin{array}{ll}
1. & 0{\cdot}0034493 - 0{\cdot}0000884\,z - 0{\cdot}0000000\,y \\
2. & 1{\cdot}9954025 - 0{\cdot}0511335\,z - 0{\cdot}0026146\,y \\
3. & 5{\cdot}2627314 - 0{\cdot}1347996\,z - 0{\cdot}0181710\,y \\
4. & 5{\cdot}9233828 - 0{\cdot}1517026\,z - 0{\cdot}0230137\,y \\
5. & 15{\cdot}2600913 - 0{\cdot}3903417\,z - 0{\cdot}1523666\,y \\
6. & 19{\cdot}2920719 - 0{\cdot}4932370\,z - 0{\cdot}2432826\,y \\
7. & 19{\cdot}4476573 - 0{\cdot}4972172\,z - 0{\cdot}2472249\,y \\
8. & 20{\cdot}0915076 - 0{\cdot}5136117\,z - 0{\cdot}2637970\,y \\
9. & 22{\cdot}1773831 - 0{\cdot}5667720\,z - 0{\cdot}3212304\,y \\
10. & 23{\cdot}6615693 - 0{\cdot}6045723\,z - 0{\cdot}3655077\,y \\
11. & 23{\cdot}9844238 - 0{\cdot}6127966\,z - 0{\cdot}3755198\,y \\
12. & 24{\cdot}4485230 - 0{\cdot}6246030\,z - 0{\cdot}3901290\,y \\
13. & 25{\cdot}2710761 - 0{\cdot}6455596\,z - 0{\cdot}4167473\,y \\
14. & 26{\cdot}8974396 - 0{\cdot}6869392\,z - 0{\cdot}4718856\,y \\
15. & 27{\cdot}9692193 - 0{\cdot}7142003\,z - 0{\cdot}5100820\,y \\
16. & 29{\cdot}8236502 - 0{\cdot}7613587\,z - 0{\cdot}5796676\,y \\
17. & 34{\cdot}8039301 - 0{\cdot}8878916\,z - 0{\cdot}7883515\,y \\
18. & 36{\cdot}5071688 - 0{\cdot}9311390\,z - 0{\cdot}8670202\,y
\end{array}
$$

Dividing the respective sums by their number $z = 39{\cdot}1478290 - 0{\cdot}6513417\,y$ (3). Equating (2) and (3).

$y = 0{\cdot}209068$, and $z = 39^{\text{in}}{\cdot}011654 = 3^{\text{ft}}{\cdot}2509712$; whence $\frac{y}{x} = 0{\cdot}005359$.

$$e = \tfrac{5}{2} \times \frac{20919576}{1856062635 \times 3{\cdot}2509712 + \psi} - \frac{y}{x}, \quad \text{or}$$

$$\frac{104597880}{12109851496} \quad 0{\cdot}005359 = 0{\cdot}0086374 - 0{\cdot}005359 =$$

$$0{\cdot}0032784 = \frac{1}{305} \qquad (4)$$

$$\text{And } l = 39{\cdot}011654 + 0{\cdot}209068 \sin^2 \lambda \qquad (5)$$

Sub-

Substituting the proper values for $\sin^2 \lambda$, we obtain the length of pendulum by computation to be compared with that from experiment.

Places.	Latitude.	Length of Pendulum by Experiment.	Length of Pendulum by Calculation.	Δ_1	Δ_2
Galapagos	0° 32′ 19″N.	39·01951	39·011672	$-0·007838\ e_1$	$+0·006792$
Madras	13 4 9	39·02339	39·022344	$-0·001046\ e_2$	$-0·000268$
San Blas	21 32 24	39·04115	39·039836	$-0·001314\ e_3$	$-0·001336$
Rio Janeiro	22 55 22S.	39·04602	39·043370	$-0·002650\ e_4$	$+0·001722$
Formentera	38 39 56N.	39·09419	39·093262	$-0·000928\ e_5$	$+0·002512$
Figeac	44 36 45	39·11319	39·114774	$+0·001584\ e_6$	$+0·001022$
Bordeaux	44 50 26	39·11300	39·115606	$+0·002606\ e_7$	$-0·001662$
Clermont	45 46 48	39·11809	39·119034	$+0·000944\ e_8$	$-0·000076$
Paris	48 50 14	39·12928	39·130148	$+0·000868\ e_9$	$-0·000317$
Dunkirk	51 2 10	39·13770	39·138051	$+0·000351\ e_{10}$	$+0·000129$
London	51 31 8	39·13929	39·139770	$+0·000480\ e_{11}$	$-0·000742$
Arbury-Hill	52 12 55	39·14250	39·142238	$-0·000262\ e_{12}$	$+0·000882$
Clifton	53 27 45	39·14600	39·146620	$+0·000620\ e_{13}$	$-0·000839$
Leith	55 58 39	39·15549	39·155271	$-0·000219\ e_{14}$	$-0·000401$
Portsoy	57 40 59	39·16159	39·160970	$-0·000620\ e_{15}$	$-0·000170$
Unst	60 26 27	39·17162	39·170830	$-0·000790\ e_{16}$	$-0·000326$
Hare Isle	70 26 17	39·19840	39·197284	$-0·001116\ e_{17}$	$+0·000441$
Melville Isle	74 47 12	39·20700	39·206325	$-0·000675\ e_{18}$	

The numbers in column Δ_1 are the differences between the results from experiment and calculation by the formula, the sign $-$ showing that the calculated lengths are less than those by experiment, and $+$ that they are greater. The numbers in column Δ_2 are obtained by subtracting those in Δ_1 successively from each other, retaining the algebraic signs.

On duly considering the quantities in Δ_1, it appears that the first five are negative; the next eight, with the exception of that at Arbury-Hill, are positive; and again, the last five are negative.

With the regularity of these signs I was forcibly struck; and after a little reflection strongly suspected that it was owing to some peculiarity in the form of the earth, more especially as these apparently show some connexion with what I formerly observed with regard to the ellipticity derived from the measurement of arcs in the corresponding latitudes. —If the figure of the earth is not that of a regular ellipsoid, what modification must it receive in order to satisfy these conditions?

It is evident, since the length of the pendulum by experiment exceeds that derived from computation on the elliptical hypothesis at Galapagos, Madras, San Blas, Rio Janeiro, and Formentera, that the gravitating force is greater at these places

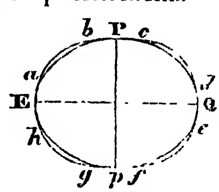
places than theory supposes. On the same principles it is less from Figeac to Clifton; and again, greater from Leith to Melville Island. The irregularity of the densities of the materials constituting the crust of the globe, combined with the small errors of observation, produce a considerable number of discrepancies, as may be remarked from consulting column Δ_2: but these appear to be insufficient to account for such marked and regular differences, especially when regard is had to their signs, as are shown in column Δ_1.

If it is not demonstrated by these observations, it is at least rendered extremely probable, that the form of the earth differs somewhat considerably from a regular spheroid.

Since from the measurement of arcs near the equator, compared with others in considerably high northern latitudes, the ellipticity is in general about $\frac{1}{310}$, while that derived from the same kind of measures at mean latitudes near the parallel of 45° N. is about $\frac{1}{150}$, nearly double of the former,—it is only necessary to give the arc of the meridian at 45° N. a greater curvature than the elliptical hypothesis supposes, to satisfy these conditions.

Again: since the length of the pendulum is shorter at 45° N. than theory requires; if a protuberance or swelling out of the earth be supposed to occur there, so as to place the experimental pendulum at a greater distance from the centre of the earth, thereby rendering the gravitating force less, while the equatorial and polar regions contract a little; then the theory would not disagree with the observed phænomena.

Thus, in the marginal figure, let P E p Q be a meridian of the earth formed by a section passing through its centre, in which Pp is the polar axis, E Q the equatorial diameter; then suppose a zone or belt passing round the globe parallel to the equator, and nearly bisected by the parallel of 45°, and of which zone, diminishing in thickness towards its boundaries, $b\,a$, $c\,d$, &c. are sections or opposite meridians, the exterior curve P$b\,e$ EpQ would be the form of the meridian, to satisfy these conditions; while the interior curve is the regular ellipse.



This conclusion must be looked upon merely as a very probable hypothesis, till by the accumulation of more numerous observations it may be either completely verified or set aside.

It is useful in the mean time to endeavour to connect observations, and reconcile these anomalies by the most probable

bable hypothesis that observation and experiment will warrant. In such circumstances, however, we cannot with propriety attempt to ascertain accurately on mathematical principles the exact form of the meridian, or whether they are all similar to one another:—this, depending on a great variety of very nice observations, must be the work of time. I can only add that the subject will not be lost sight of; and as materials increase, efforts shall be repeatedly made to arrive at the truth.

As the method of ascertaining the length of the pendulum proposed by Capt. Kater is now, from its simplicity and accuracy, much used, it appeared to me that a few formulæ, of easy application to the necessary reductions, would not be unacceptable.

On the small Corrections of Pendulums, on account of the minute Variations they may be supposed to undergo from Change of Temperature, Latitude, &c.

If $N=86400$ be the number of oscillations of the pendulum L in a mean solar day; and n, not differing much from N, the number of oscillations of a given pendulum l, nearly equal in length to L; we can easily find approximations to these quantities for small differences.

From the common treatises on mechanics we have the time $t = \frac{N}{n}$, consequently $\frac{N}{n} = \sqrt{\frac{l}{L}}$, and $l = \frac{LN^2}{n^2}$. Now suppose the pendulum L to be increased by the small quantity ΔL to find ΔN, the number of seconds N will be diminished, or those the pendulum will lose in a day.

In this case let $L + \Delta L = \frac{LN^2}{(N-\Delta N)^2} = L + \frac{2L\Delta N}{N}$ nearly,

hence $\Delta L = \frac{2L\Delta N}{N} = \frac{L\Delta N}{\frac{1}{2}N}$ (1)

and $\Delta N = \frac{N\Delta L}{2L} = \frac{\frac{1}{2}N\Delta L}{L}$ (2)

It may be observed, that the same formulæ can be applied when N is increased, and consequently L diminished.

Again: let δL be the variation of L for one degree of the thermometer to compute the change of L or N, then on this account $\Delta L = n\delta\Delta L \times L$ (3)

and $\Delta N = \frac{\frac{1}{2}N\Delta L}{L} = \frac{\frac{1}{2}Nn\delta L \times L}{L} = \frac{1}{2}Nn\delta L$ (4)

n here being the number of degrees of difference of temperature.

According to the mean of a number of experiments upon various kinds of brass, its lineal variation from the freezing to the boiling

boiling point is $=0{\cdot}0018709$ part of itself. This gives for every degree of Fahrenheit $\frac{0{\cdot}0018709}{180}=0{\cdot}0000104$ part of itself, or $0{\cdot}00001$ nearly the value of δL, which agrees sufficiently well with Capt. Kater's experiments.

Substituting this in formula (3) it becomes $\Delta L=\frac{nL}{100000}$ (5); or, in words, shift the decimal point in the length L five places to the *left*, and multiply by the number of degrees of change of temperature, the result will be the expansion at the rate we have mentioned; otherwise the actual variation by experiment from formula (3) must be employed. If the value of ΔL formula (5) be subtracted in formula (4), we get

$$\Delta N=\frac{\frac{1}{2}Nn}{100000} \qquad (6)$$

Or shift the decimal point in $\frac{1}{2}$N five places to the *left*; this result, multiplied by the number of degrees of change of temperature, will give the correction required.

If N do not differ much from 86400, formula (6) would become $\Delta N=0{\cdot}432n$ (7)
And this may be considered as sufficiently accurate, unless n, the number of degrees of change of temperature, be considerable, or N differ above 100 seconds from 86400, the expansion for each degree of Fahrenheit's thermometer remaining the same.

To exemplify these, let us suppose $n=6°$ Fahrenheit; then by formula (7) $\Delta N=0{\cdot}432\times 6=2^{s}{\cdot}592$, the retardation or acceleration in a day for that expansion in a brass pendulum.

At 62° Fahrenheit, Capt. Kater found the pendulum sent out with Capt. Hall made 86235·98 oscillations in a day: it therefore, from an expansion answering to 6° of Fahrenheit's thermometer, would be retarded $2^{s}{\cdot}592$, and $86235^{s}{\cdot}98-2^{s}{\cdot}59=86233^{s}{\cdot}39$, the number it would actually perform in the same place at a temperature of 68°.

Now, by our ordinary treatises on mechanics $l:l'::n^2:n'^2$ we have, since Capt. Hall found the same pendulum made 86101·34 oscillations in a day at the Galapagos $(86233{\cdot}39)^2:(86101{\cdot}34)^2::39^{in}{\cdot}13929:39^{in}{\cdot}019514$, the length of the pendulum oscillating seconds at the Galapagos at 62° of Fahrenheit, that at London being $39^{in}{\cdot}13929$. This operation, however, is tedious.

The formula will give an approximation to this for $\Delta N=86233{\cdot}39-86101{\cdot}34=132{\cdot}05$. Hence from formula (1) $\Delta L=\frac{L\times\Delta N}{\frac{1}{2}N}=\frac{39{\cdot}13929\times 132{\cdot}05}{43116{\cdot}7}=-0{\cdot}11987$.

Hence $39{\cdot}13929-0{\cdot}11987=39{\cdot}01942$, which differs from the

the former only $0^{in}\cdot000094$, or about $\frac{1}{10\cdot000}$ part of an inch. These approximating rules, when ΔN is great, cannot be employed where extreme accuracy is required. They will be sufficiently correct when ΔN is small, as in the case of determining the length of the pendulum at various points on an arc of the meridian not differing above a degree or so from each other. If, however, the mean of the numbers of oscillations at the two places be used, the results would in general be more correct; and formulæ (1) and (4) may always be employed when the difference of the numbers of oscillations at the two places does not exceed 30 or 40.

To render the results accurate in all probable cases when the formulæ are used, we have computed the following Table of corrections for various differences in the number of oscillations.

Variation of Oscillations in 24 hours.	Correction I.	Differences.	Correction II.
oscil.			
1	0·00000001	0·00000001	
2	0·00000002	0·00000003	
3	0·00000005	0·00000003	
4	0·00000008	0·00000005	
5	0·00000013	0·00000006	
6	0·00000019	0·00000007	
7	0·00000026	0·00000007	
8	0·00000033	0·00000009	
9	0·00000042	0·00000010	
10	0·00000052	0·00000157	0·00000000
20	0·00000209	0·00000261	0·00000001
30	0·00000470	0·00000366	0·00000001
40	0·00000836	0·00000470	0·00000002
50	0·00001306	0·00000575	0·00000003
60	0·00001881	0·00000679	0·00000005
70	0·00002560	0·00000784	0·00000006
80	0·00003344	0·00000888	0·00000008
90	0·00004232	0·00000993	0·00000010
P. P. for sec. diff.	os. 1 2 3	4 5 6	7 8 9
after 10 oscil.	cor. 5 8 11	12 13 12	11 8 5

Variation of Oscillations in 24 hours.	Correction I.	Differences.	Correction II.
os.			
100	0·00005525	0·00001097	0·00000013
110	0·00006322	0·00001202	0·00000015
120	0·00007524	0·00001306	0·00000018
130	0·00008830	0·00001411	0·00000021
140	0·00010241	0·00001515	0·00000025
150	0·00011756	0·00001620	0·00000029
160	0·00013376	0·00001724	0·00000033
170	0·00015100	0·00001828	0·00000038
180	0·00016928	0 00001933	0·00000043
190	0·00018861	0·00002037	0·00000048
200	0·00020898	0·00002142	0·00000053
210	0·00023040	0·00002246	0·00000058
220	0·00025286	0·00002351	0·00000064
230	0·00027637	0·00002455	0·00000070
240	0·00030092	0·00002560	0·00000077
250	0·00032652	0·00002664	0·00000084
260	0·00035316	0·00002769	0·00000090
270	0·00038085	0·00002873	0·00000098
280	0·00040958	0·00002978	0·00000105
290	0·00043936	0·00003083	0·00000113
300	0·00047019	0·00003187	0·00000121
310	0·00050206	0·00003292	0·00000129
320	0·00053498	0·00003396	0·00000138
330	0·00056894	0·00003501	0·00000147
340	0·00060395	0·00003605	0·00000156
350	0·00064000	0·00003710	0·00000165
360	0·00067710	0·00003814	0·00000175
370	0·00071524	0·00003918	0·00000184
380	0·00075442	0·00004022	0·00000194
390	0·00079464	0·00004126	0·00000204
400	0·00083590		0·00000215

In this table, column first contains the difference of the number of oscillations made by the experimental pendulum at two different places;—column second contains the correction of the formula, or its deviation from the result deduced from the method of obtaining the length of the pendulum by the squares of the number of oscillations when the length at the first place of observation is 39 inches; and is always to be subtracted;—column third contains the differences to obtain proportional parts readily;—and column fourth contains the correction

correction to be applied for a variation in the length of the pendulum of one-tenth of an inch, or when it is increased from 39 to 39·1 inches; and is always to be added. By means of these it is hoped the length of the pendulum can with sufficient accuracy be more easily obtained than by using the laborious process of the squares of the numbers of oscillations, as may be seen by the following examples.

Capt. Kater found that his experimental pendulum at London, in latitude 51° 31′ 8″ N., after the proper reductions made 86061·52 oscillations in a mean solar day at 62° Fahrenheit; while at Unst, in latitude 60° 45′ 28″ N., the same pendulum, at the same temperature, made 86096·90 oscillations. Required, the length of the seconds pendulum at Unst, that at London being 39·13929 inches?

Number of oscillations at Unst . . .	86096·90
at London . .	86061·52
Difference more	35·38

Hence the seconds pendulum must be longer at Unst, and the general correction must be added.

Now by formula (1) $\Delta L = \frac{L\Delta N}{\frac{1}{2}N}$, which by substituting the proper quantities stated above becomes

$+\frac{39{\cdot}13929 \times 35{\cdot}38}{43048{\cdot}45} =$		+0·03216697
Correction from table, col. 2, for 30$^{os\cdot}$	−470	
Prop. part for 5$^{os\cdot}$·38, col. 3, . . .	−197	
Equation for 2d difference, foot of table,	+ 13	
Correction for +0·13929, col. 4,	− 2	
Amount	−656	656
Total correction to be added		+ 0·03216041
Length of seconds pendulum at London .		39·13929
Length of the pendulum at Unst		39·17145

differing only one unit in the fifth decimal place from the determination of Capt. Kater.

In the application of the corrections of the formula, the equation of second difference and that from column 4 are applied as they ought to be to the first part of the correction from column 2, with contrary signs. Indeed, it is unnecessary to carry them further than about five or six places of decimals, being more than even the best observations can warrant, as may be readily seen by comparing those of Kater and Biot at the same place; as for example, at Leith or Unst. In fact, without the small corrections, the formula in this case would have given Kater's determination exactly.

Again: Capt. Hall found that an experimental pendulum, making

making 86235·98 oscillations in a mean solar day at London, at the temperature of 62° Fahrenheit, made 86101·34 oscillations at the Galapagos Isles, at the temperature of 68° Fahrenheit.

Number of oscillations at London	86235·98
Correction for 68°, or 6° more than 62° $=\cdot 4312 \times 6°$ formula (6)	— 2·59
Number of oscillations at 68°	86333·39
Number at Galapagos at 68°	86101·34
Difference less	132·05

Hence the seconds pendulum must be shorter than that at London.

Whence by formula (1), as before,

$$\text{we have} \quad \Delta L = \frac{39\cdot13929 \times 132\cdot05}{43116\cdot7} = -0\cdot11986870$$

Correction from table, col. 2, for 130$^{os.}$	—8830	
Prop. part for 2·05$^{os.}$, col. 3, . . .	— 289	
Equation, second difference . . .	+ 8	
Correction, col. 4, ...	— 5	
Amount	—9106	9106
Total correction		— 0·11977764
Length of pendulum at London		39·13929
Length at Galapagos		39·01951

At the temperature of 62°, or that at which the length of the pendulum at London =39·13929 was obtained; and so on in similar cases.

Appendix.

The French mathematicians generally give the length of the pendulum according to their new system of weights and measures. In this case the day is supposed to be divided into 100,000 seconds, instead of the 86,400 seconds according to old custom.

Now, since the lengths of pendulums are inversely as the squares of the number of oscillations in the same time; and the length of the standard metre at the freezing point, compared with Sir George Shuckburgh's standard scale at 62° Fahrenheit, is, according to the accurate determination of Capt. Kater, 39·37079 inches; we can convert the French measures readily into ours by the following formula:

$$L = \left(\frac{1000}{864}\right)^2 \times l \times 39\cdot37079 = 52\cdot74079\, l \qquad (8)$$

of Sir George Shuckburgh's scale; or

$$L = 52\cdot740564\, l \qquad (9)$$

of Bird's parliamentary standard of 1758, which is equal to 36·00016

36·00016 inches of Sir George Shuckburgh's scale, in which Capt. Kater's measures were originally taken.

In these L is the length of the sexagesimal pendulum, and l that of the decimal metrical pendulum.

These have been added to this paper for the convenience of comparing the British and French measures.

I am, gentlemen, yours &c.

Edinburgh, Nov. 11, 1824. WILLIAM GALBRAITH.

IV. *On the Annual Secular Changes in the Solar System.* By A CORRESPONDENT.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE inclosed paper is offered for the purpose of promoting inquiry on a point materially affecting the accuracy of the solar tables: if it appear to you worthy the attention of your readers, you will oblige the author by inserting it in your valuable Journal.

I am, gentlemen, yours &c.

C.

Having remarked that a secular diminution of 42″·67 answered better than any other I had tried for the secular change of the obliquity of the ecliptic, I was induced to examine whether the masses of Venus and Mars, altered to suit it, would represent with any degree of accuracy the annual and secular changes in the other phænomena of the solar system proceeding from the action of the planets. I accordingly collected all the observations of the nodes and aphelia of the several planets' orbits I could procure, and from their comparison estimated as nearly as I could the annual change in each. I then calculated, upon Laplace's hypothesis of the masses of the planets, their respective perturbations from the formulæ in Woodhouse's Astronomy, and then changed those of Venus and Mars to suit my hypothesis of the change in the obliquity; and the close coincidence of the results with observation have determined me to lay them before the readers of your valuable Journal.

The following are the principal of the observations which I procured, many of them having been rejected for giving results differing much from the mean; but upon the whole I cannot say that I place too much reliance on any of them; so that when my calculations give a number within the two extremes

tremes of those deduced from observation, I rest perfectly satisfied. The observations are,

For Mercury's Node.

				s	°	′	″		″
1. Gassendi	1631	Nov. 7,	1	13	30	47	10—8	41·98
2. Hevelius	1661	May 3,	1	14	19	0	7	44·82
3.	1677		1	14	21	3	6	40·78
4. Halley	1723		1	15	0	53	5	40·84
5. Cassini	1731		1	15	10	0	4	43·60
6.	1736	Nov. 11,	1	15	14	5	3	45·13
7. (Transit)	1736	Oct. 26,	1	15	9	34	2	41·02
8.	1753		1	15	24	14	6—2	43·70
9. Rumovski	1786	May,	1	15	53	56		———
10. Zach	1802	Nov. 8,	1	15	58	56·3	Mean	42·73

For Venus's Node.

			s	°	′	″		″
1. Horrox	1639		2	13	27	50	7—4	31·95
2. Cassini	1698		2	14	1	45	1	31·28
3. Cassini	1705	June 11,	2	14	2	52	6—4	32·53
4. La Caille	1746	Dec. 21,	2	14	23	10	3	30·86
5. Lalande	1769	July 3,	2	14	36	20	1	31·22
6. Bugge	1786	Aug. 25,	2	14	44	38	5—3	31·33
7. Mersier	1787	Aug.	2	14	45	19	1	31·49
							Mean	31·52

Lalande thinks 56″ answers best for the aphelion of Mercury; but that of Venus has hitherto defied all certainty, on account of the little eccentricity of her orbit, the results varying from 2½ minutes to 40″.

For Mars's Aphelion.

			s	°	′	″		″
1. Cassini	1592		4	28	49	50	5—4	66·20
2. Cassini	1600		4	23	24	24	3	63·00
3. Flamsteed	1696		5	0	31	34	2	60·13
4. Lalande	1743	Feb. 15,	5	1	20	39	4—3	62·60
5.	1748		5	1	26	10		———
							Mean	63·00

For Mars's Node.

			s	°	′	″		″
1. Tycho Brahé	1595	Oct. 28,	1	16	24	33	1—7	28·66
2. Flamsteed	1700	May 7,	1	17	24	13	6	30·25
3. Cassini	1700	May 6,	1	17	13	43	5	29·44
4. Cassini	1721	Nov. 13,	1	17	29	49	4	31·08
5. La Caille	1753	Nov. 4,	1	17	42	5	½(3+2)	31·24
6. Maskelyne	1778	April 17,	1	17	51	40		———
7. Bugge	1783	Dec. 7,	1	17	54	24	Mean	30·134

The

The motion of Jupiter's aphelion and Saturn's, since they arise almost entirely from the action of each other, and as their masses are best determined from their mutual periodical inequalities, it is almost useless to examine the motions of their aphelia from observation: the motion of their nodes, however, depends in a great measure on Venus and Mars; we shall therefore examine them.

For Jupiter there is still some uncertainty; however, Delambre makes it about 35″·7, with a possible error of 2 or 3 seconds. Lalande employs this motion in his Tables. For Saturn, Delambre makes the motion 33″·35: Lalande, 31″·7. The mean of these, since they agree so nearly, cannot be far from the truth; we shall therefore suppose it 32″·52. We have therefore the following number:

	Mercury.	Venus.	Earth.	Mars.	Jupiter.	Saturn.
Node...	42″·73	31″·52	——	30″·13	35″·7	32′·52
Aphelion	56″	——	61″·908	63″·00	——	——

Laplace's values for the masses, which I first assumed as a means of examining the above numbers, are as follows:

Mercury	$\frac{1}{2025810}$	Jupiter	$\frac{1}{1067 \cdot 019}$
Venus	$\frac{1}{356632}$	Saturn	$\frac{1}{3534 \cdot 08}$
Earth	$\frac{1}{342146}$	Georgian	$\frac{1}{19504}$.
Mars	$\frac{1}{2546320}$		

These are all extracted from Laplace, with the exception of that of the Earth, which he has estimated too largely, by assuming the parallax 8″·81 instead of 8″·56, which his own lunar theory and the calculations of Short from the last transit point out. I however, for various reasons, have assumed it 8″·7. Laplace says, Mercury's, Venus's and Mars's masses were first determined from their densities upon Lagrange's hypothesis of their being inversely proportional to their distance from the sun, and then corrected from observation. He therefore diminishes Mars's mass so found, in the ratio of ·725 to unity from Delambre's observations of its effect on the periodical inequalities of the Earth's motion. And from similar observations he increases Venus's mass determined from a secular diminution of the obliquity of 50″, in the ratio of 1·0743 to unity. And finally, Saturn's was first settled at $\frac{1}{3359 \cdot 4}$, as an elongation of 2′ 57″ of the fourth satellite required; and then the inequalities in the motion of Jupiter upon this supposition, compared with fifty oppositions, showed that it must be diminished by $\frac{1}{20232}$, which reduced it to $\frac{1}{3534 \cdot 08}$, the above value.

With these values we have the following secular motions of the planets' orbits:

Sidereal Motion of the Perihelia.

By the Action of	Mercury.	Venus.	Earth.	Mars.	Jupiter.	Saturn.	Georgian.
Mercury		−4·315	−·415	−·016	·000	·000	·000
Venus	3·238		3·022	·549	·005	·001	·000
Earth	·896	−5·544		2·051	·009	·000	·000
Mars	·030	·873	1·121		·001	·000	·000
Jupiter	1·560	6·436	6·804	12·313		15·791	1·211
Saturn	·076	·080	·184	·660	6·139		1·182
Georgian	·002	·003	·007	·014	·125	·320	
Real Mo.	5·802	−2·467	10·723	15·571	6·279	16·111	2·393
Precesⁿ.	50·100	50·100	50·100	50·100	50·100	50·100	50·100
Ann. Mo.	55·902	47·633	60·823	65·671	56·379	66·211	52·493

Sidereal Motion of the Nodes.

By the Action of	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
Mercury	− ·098	+ ·165	− ·318	− ·316	− ·111	− ·789
Venus	− 4·356	−5·830	− 9·215	−13·782	− 6·320	−25·585
Earth	− ·929	−7·145	− 1·893	− ·009	− ·001	− ·000
Mars	− ·104	− ·208	− ·314	− ·282	− ·103	− ·681
Jupiter	− 2·187	−5·133	−11·016	− 6·948	−12·293	−10·016
Saturn	− ·112	− ·271	− ·446	+ 5·587	− ·323	+ 1·281
Georgian	− ·002	− ·005	− ·011	− ·049	− ·271	− ·007
Real Mo.	− 7·788	−18·427	−23·213	−13·799	−19·422	−35·797
Precesⁿ.	50·1	50·1	50·1	50·1	50·1	50·1
Ann. Mo.	42·312	31·673	26·887	36·301	30·678	14·303

The obliquity of the ecliptic is deduced from the following calculation (the epochs being taken from Woodhouse's Astronomy, vol. i. p. 286):

Mercury	·097574	tan. 7° 0′ 1″	sin. 45°57′ 31″ .	·008612
Venus	5·829900	tan. 3 23 32	sin. 74 52 38·6.	·333595
Mars	·313925	tan. 1 51 3·6	sin. 48 14 38 .	·007568
Jupiter	6·947861	tan. 1 18 51	sin. 98 25 34 .	·157640
Saturn	·322870	tan. 2 29 34·8	sin. 111 55 46	·013040
Georgian	·007096	tan. 0 46 26	sin. 72 51 14	·000092
			Entire annual diminutⁿ.	·520547

Now

Now this annual diminution of Laplace's, and which Delambre uses in his Tables of the Sun, is considerably too great, and will not at all agree with modern observations. I have in the following table compared the observed obliquity with that deduced from it, and also with that from an annual diminution of ·42667, the number which I think answers better than any other I have tried.

		°	′	″	°	′	″		°	′	″	
Ulubeg	1437	23	30	27	23	31	6·0	+39·0	23	30	31·9	+4·9
Tycho	1587	23	29	30	23	29	47·9	+17·9	23	29	27·9	−2·1
Cassini	1672	23	28	54	23	29	3·6	+ 9·6	23	28	51·6	−2·4
Richers	1672	23	28	51·5	23	29	3·6	+12·6	23	28	51·6	+0·1
Condamine ...	1736	23	28	24	23	28	30·3	+ 6·3	23	28	24·4	+0·4
De La Caille, Bradley, Mayer	1750	23	28	18·3	23	28	23·0	+ 4·7	23	28	18·3	+0·0
Brinkley	1755	23	28	15·5	23	28	20·4	+ 4·9	23	28	16·2	+0·7
Maskelyne......	1769	23	28	10·0	23	28	13·1	+ 3·1	23	28	10·2	+0·2
Philoso. Trans.*	1772	23	28	8·7	23	28	11·6	+ 2·9	23	28	8·9	+0·2
Lalande	1786	23	28	0·0	23	28	4·3	+ 4·3	23	28	3·0	+3·0
Cassini	1788	23	27	58·6	23	28	3·2	+ 4·6	23	28	2·1	+3·5
Delambre, &c.	1800	23	27	57·0	23	27	57·0	+ 0·0	23	27	57·0	−0·0
Oriani, Pond, Brinkley, Arago	1813	23	27	50·5	23	27	50·2	− 0 3	23	27	51·4	+0·9

Thus we see that 230 years ago Laplace's formula errs 39″, while the greatest error arising from the assumption of ″·42667 is only 4″·9; we may therefore consider it nearly correct. The progression of the perihelion of the Earth's orbit came out, as we have seen above, 10″·723; but Delambre from the comparison of a great number of observations makes it 11″·808: I have therefore assumed 42″·667 and 11″·808 to be the exact values for the changes in the obliquity and in the longitude of the perihelion, and afterwards calculated the multipliers of Venus's and Mars's masses upon two suppositions; the one that Mercury's mass is what Laplace supposes it, and the other that it is equal to nothing; and then examined which result agreed with observation.

If we take Mercury's mass what Laplace supposes it, we have the two following equations, to determine the multipliers for Venus's and Mars's mass (that is the true mass of Venus $\frac{1}{356632}x$, and Mars $\frac{1}{2546320}y$: then)

$$3022\,x+1121\,y=5228$$
$$3336\,x+75{\cdot}68\,y=2473$$

whence $x=\cdot676905$ and $y=2{\cdot}8389$. But if we suppose Mercury's mass $=0$, then the expressions become

$$3022\,x+1121\,y=4813$$
$$3336\,x+75{\cdot}68\,y=2559$$

whence $x=\cdot71331$ and $y=2{\cdot}371$. On these two suppositions the following tables have been constructed.

* See Philosophical Transactions, 1773, page 93.

Motion

Motion of the Perihelia.

1st Supposition.

By the Action of	Mercury.	Venus.	Earth.	Mars.	Jupiter.	Saturn.	Georgian.
Mercury		−4·315	−·415	−·016	·000	·000	·000
Venus	2·192		2·046	·372	·004	·001	·000
Earth	·896	−5·544		2·051	·009	·000	·000
Mars	·085	2·549	3·182		·003	·000	·000
Jupiter	1·560	6·436	6·804	12·313		15·791	1·211
Saturn	·076	·080	·184	·660	6·139		1·181
Georgian	·002	·003	·007	·014	·125	·320	
Sider.mo.	4·811	−0·791	11·808	15·394	6·280	16·111	2·393
Precesⁿ.	50·1	50·1	50·1	50·1	50·1	50·1	50·1
Ann. mo.	54·911	49·309	61·908	65·494	56·380	66·211	52·493

2d Supposition.

By the Action of	Mercury.	Venus.	Earth.	Mars.	Jupiter.	Saturn.	Georgian.
Mercury		− ·000	−·000	−·000	·000	·000	·000
Venus	2·310		2·156	·392	·004	·001	·000
Earth	·896	−5·544		2·051	·009	·000	·000
Mars	·071	2·070	2·658		·003	·000	·000
Jupiter	1·560	6·436	6·804	12·313		15·791	1·211
Saturn	·076	·080	·184	·660	6·139		1·181
Georgian	·002	·003	·007	·014	·125	·320	
Sider.mo.	4·915	3·045	11·809	15·430	6·280	16·111	2·393
Precesⁿ.	50·1	50·1	50·1	50·1	50·1	50·1	50·1
Ann. mo.	55·015	53·145	61·909	65·530	56·380	66·211	52·493

Motion of the Nodes.

1st Supposition.

By the Action of	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
Mercury	− ·098	+ ·165	− ·318	− ·316	− ·111	− ·789
Venus	−2·949	− 3·946	− 6·238	− 9·329	− 4·278	−17·319
Earth	− ·929	− 7·145	− 1·893	− ·009	− ·001	− ·000
Mars	− ·295	− ·591	− ·891	− ·800	− ·292	− 1·988
Jupiter	−2·187	− 5·133	−11·016	− 6·948	−12·293	−10·016
Saturn	− ·112	− ·271	− ·446	+ 5·587	− ·323	+ 1·281
Georgian	− ·002	− ·005	− ·011	− ·049	− ·271	− ·007
Sider.mo.	−6·572	−16·926	−20·813	−11·865	−17·569	−28·838
Precesⁿ.	50·1	50·1	50·1	50·1	50·1	50·1
Ann. mo.	43·528	33·174	29·287	38·235	32·531	21·262

2d Sup-

2d Supposition.

By the Action of	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
Mercury	− ·000	+ ·000	− ·000	− ·000	− ·000	− ·000
Venus	−3·107	− 4·159	− 6·573	− 9·831	− 4·508	−18·250
Earth	− ·929	− 7·149	− 1·893	− ·009	− ·001	− ·001
Mars	− ·247	− ·493	− ·745	− ·669	− ·245	− 1·615
Jupiter	−2·187	− 5·133	−11·016	− 6·948	−12·293	−10·016
Saturn	− ·112	− ·271	− ·446	+ 5·587	− ·323	+ 1·281
Georgian	− ·002	− ·005	− ·011	− ·049	− ·271	− ·007
Sider. mo.	−6·584	−17·206	−20·684	−11·919	−17·641	−28·607
Precesⁿ.	50·1	50·1	50·1	50·1	50·1	50·1
Ann. mo.	43·516	32·894	29·416	38·181	32·459	21·493

From the comparison of these results with those procured from observation, I am induced to give the preference to the latter supposition, or to suppose that the mass of Mercury is *very* inconsiderable: the errors of the second hypothesis are; for Mercury's aphelion and node, $+''{\cdot}98$ and $-''{\cdot}79$; for Mars's, $2''{\cdot}53$ and $-''{\cdot}71$; for Venus's, Jupiter's, and Saturn's nodes, $+1''{\cdot}37$, $2''{\cdot}5$, and $-''{\cdot}06$ respectively, which are at least as trifling as the nature of the subject will admit of. As the mass of Mercury can only be determined from its effect on the motion of Venus's perihelion, I am at present engaged in determining the elements of her orbit from some observations in my possession, which I think will enable me to determine the motion with considerable accuracy. In the mean time I may state that, from using $\frac{1}{300000}$ and $\frac{1}{1073944}$ for the masses of Venus and Mars, most of the annual changes of the orbits of the planets are deduced with as great accuracy as from Laplace's, and some of them with greater.

The change in the obliquity from each hypothesis is

Mercury	·00861	·00000
Venus	·22581	·23796
Mars	·02149	·01794
Jupiter	·15764	·15764
Saturn	·01304	·01304
Georgian	·00009	·00009
	·42667	·42667

The inclinations of their orbits to the ecliptic are subject to the following changes: Mercury $''{\cdot}158$, Venus $''{\cdot}019$, Mars $-''{\cdot}050$, Jupiter $-''{\cdot}190$, Saturn $-''{\cdot}106$ and the Georgian $''{\cdot}023$.

V. *Answer*

V. *Answer to Mr.* J. LINDLEY'S *Letter on the Subject of Vegetable Physiology**. *By Sir* JAS. EDW. SMITH, *F.R.S. &c. President of the Linnæan Society.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I CANNOT but respect that independance of character which Mr. J. Lindley avows, in refusing to sacrifice the interests of science to the personal pretensions of any one. I trust that no such sacrifice has ever been looked for on my part; nor should I honour the man who would offer it. But I do not see how the interests of science are concerned in this gentleman's charge of inaccuracy in my statements.

My pretensions are simply these—that the theory of vegetation, first published in my Introduction to Botany in 1807, is original. Dr. Darwin's experiments and observations first led me to an acquaintance with the true sap-vessels, or arterial and venous system, of Plants, previously taken for their air-vessels and lungs; and Mr. Knight's more extensive and luminous remarks, independent of Dr. Darwin's, but essentially confirming them, led me to reconsider the subject of vegetable physiology. With the theory of Du Humel, to which neither of those writers, nor even Mr. Lindley, adverts, though the only one previously extant, I had always been dissatisfied; and while the real sap-vessels remained unknown, the great chemical discoveries of Priestley and others, respecting the effects of air and light upon the vegetable body, could not be properly understood. But when all these inquiries and their results were brought together under one view, and the vital principle of my great teacher, Mr. John Hunter, was taken into consideration, it was not difficult to derive from the whole a sufficiently plain and consistent theory of vegetable growth, respiration, secretion, &c. To this Theory I lay claim as original, with the illustration of all the particulars concerning the leaves, flowers and fruit, as far as they are dependent upon it, and which had never before been explained, such as the fall of the leaf, but more especially the "flowing of the sap," as it is called, which had occupied so much of the attention of every previous inquirer, without being understood by any one. Whatever obligation I may avow, as I do, to the facts and experiments of others, the theory built upon them is my own, and competent judges have never denied me the credit of it. I am obliged to be thus decisive, because Mr. Lindley has, in a very unworthy manner, attempted to represent me as giving up my own right. If

* Phil. Mag. vol. lxiv. p. 456.

If he believed I had no such right, he ought to have given the honour where he supposed it to be due. To publish the opinions of M. du Petit-Thouars as entirely novel in this country, betrays what I should hope is merely an unacquaintance with this branch of science and literature. I am flattered to find these opinions so powerfully confirm mine. But whether or not this truly eminent philosopher agrees with me in every detail, or whether, as it appears, he has carried his inquiries further, and made more very interesting remarks, than I profess to have done, especially concerning Buds, I hope I may be excused for attending to other things. Practical Botany is my pursuit, and Nature my book. I have known in my time the "celebrated philosophers" of France, and their "violent opposition" to one another. How they "cover themselves with glory," and how Englishmen, who have little credit at home, cover themselves likewise with what they can borrow. Mr. Lindley's character and abilities are above all this, and I hope he thinks as well of mine. If I have differed from him about *Reseda* and *Mespilus*, in my *English Flora*, I surely have not mentioned him in a way to give offence, and should be very sorry if he has taken any. I have found it necessary for "the interests of science" to differ likewise from M. Richard, a truly eminent botanical philosopher, though a great corruptor of our terminology, as his translator is aware. Some of these differences I have mentioned, with all due respect, in a new edition of my Introduction, now in the press, and they have no connexion with the matters in dispute between me and Mr. Lindley. Though I saw his translation while printing, I either did not see, or till lately did not advert to, his preface, the first paragraph of which is as follows:

"Among the number of elementary works which have issued from the English press within a few years, it is to be lamented that *not one* should have appeared, which is at all equal to explain one of the most important parts of botany, the structure of fruits and seeds." Mr. Lindley thus further enforces this assertion in his letter in your last Number, p. 457:

"I said that with reference to the subject of the work in question, that is, of fruits and seeds, *nothing* had been done in the form of an *elementary work*. For the truth of such a statement, I appeal to the world."

Having, to the best of my abilities, furnished the English student of botany with all the information on the above subject requisite for practical use, always preferring, as in all my botanical characters, what is apparent and clear, to what, though *seeming learned*, is most obscure, difficult and uncertain, I cannot but feel the above as a personal and unmerited attack.

attack. I should however have left it, as I now do, to the decision of every reader. But the sequel of Mr. Lindley's preface does so much injustice to all who have pursued botany in this country, and so absurdly contrasts the Natural Orders of Jussieu (chiefly indeed those of Linnæus) with the artificial system of Linnæus, things which no experienced or learned botanist could ever put in competition, that I could not but be struck with the injustice of the representation altogether. On this subject I also "appeal to the world," and having done so, I shall return to my own occupations. Having been the first to make known to my countrymen, in two different works, the transcendent merits of my illustrious and constant friend M. De Jussieu, I cannot be reproached with insensibility to his worth, and I am sure he would be one of the last to charge me with it.

Hoping, gentlemen, that I shall have no further occasion to trouble you on any controversial subjects,

I remain, your constant reader,

Norwich, Jan. 18, 1825. J. E. SMITH.

VI. *A Letter in reply to the Historical Sketch of the Problem of Atmospherical Refraction in the last Number of the Journal of Science.* *By* J. IVORY, *Esq. M.A. F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I ADDRESS you to request you will insert in your next publication the following observations, occasioned by an historical sketch of the problem of Atmospherical Refraction that appeared in the last Number of the Journal of Science edited at the Royal Institution.

1. It is said, p. 375, that the exponent $\frac{5}{4}$ which I have chosen in my solution of the problem, is derived from observations on mountains at small heights. This is incorrect. In the average I have adopted, all the greatest known heights are taken into account, not excepting Gay-Lussac's ascent. The result obtained is then compared to the same ascent, which is the greatest height hitherto attained by man; and the difference is shown to be very inconsiderable.

Great stress is laid upon the extreme accuracy of the French Horizontal Refraction. Laplace has adopted this quantity as a mean of the results of astronomers, although he allows that the quantity of this element has been very imperfectly ascertained. If we turn to the *Tables Astronomiques*, 1806, we shall find the horizontal refractions adopted by six eminent astronomers;

nomers; the greatest being that of Laplace 2026″, and the least that of Mayer 1885″; and M. Bessel now makes the same quantity amount to more than 2160″ by direct observation. Therefore all arguments must lose their force, which suppose that no variation can take place in an element so imperfectly known.

But it would be futile to scrutinize minutely the observations of an author whose chief solicitude is to say every thing that will at all make for his purpose. He glides along, heaping assertion upon assertion, without much minding the firmness of the ground on which he treads *.

2. In p. 376, I am charged with a mistake and a paradox. The reason assigned is a very extraordinary one. The mistake is said to arise from correcting the expression of the pressure so as to be $=1$ at the surface of the earth, a procedure, it is thought, which is not necessary. Now it is clear that the pressure and density, denoted by the symbols y and z, do not mean the absolute, but the relative quantities; that is, the proportions of the pressure and density at any height in the atmosphere to the like quantities at the earth's surface. Therefore, in ascending from the earth, we necessarily have initially $y = 1$, $z = 1$.

In the hypothesis of Kramp and Bessel, the rigorous quantities are

$$y = c^{-\frac{1}{e}(c^{es}-1)}$$

$$z = c^{-\frac{1}{e}(c^{es}-1)+es};$$

but the expression of the density is made more simple; and the quantities used in calculation are as follows:

$$y = c^{-\frac{1}{e}(c^{es}-1)}$$

$$z = c^{-(1-e)s};$$

the function for heat being c^{-es}. In order to show how nearly one density is numerically equivalent to the other, M. Bessel gives a table of the values of both computed for the same values of s, extending the calculation to a great height in the atmosphere.

* It is said, p. 378, that I had discovered that the French Tables were computed for the freezing temperature. I made no discovery. The construction of the tables is very fully explained by Delambre in the *Tables Astronomiques*, 1806. The whole of the passage relating to the corrections, p. 378, is very strange.

Now in all this it is presumed that the substituting of one density for the other will produce no material alteration in the atmosphere in other respects; not reflecting that a change in the expression of the density, although numerically insensible, by varying both the pressure and the function for heat, may entirely alter the nature of an atmosphere, as happens in the present instance. When the density is $c^{-(1-e)s}$, the rigorous expressions of the pressure and the function for heat are respectively,

$$\frac{c^{-(1-e)s}}{1-e} - \frac{e}{1-e}$$

$$\frac{1}{1-e} - \frac{ec^{(1-e)s}}{1-e},$$

by which it is proved that the properties of the new atmosphere are entirely different from those of the one originally assumed.

Thus there is neither mistake nor paradox in my paper, although there is some inconsistency in the calculations of Kramp and Bessel.

3. It is said, p. 377, that my table of refractions has been examined in a former Number of the Journal. Now here the word *examine* must be understood in a sense different from its usual import in the English language. The author selects a set of observations by Mr. Groombridge; he then alters his table in various respects so as to reduce the errors of the observations to the least possible quantities, that is, so that the errors with opposite signs may be equal in amount. Having made the most of his table, he compares his manufactured results with the errors of the same observations computed simply by my table; and in this manner he makes out that one table has little to boast of over the other. But, since they cannot be compared when put on a par, every candid examiner, following the rules of an unsophisticated logic, must conclude that there is no comparison between them. What he is pleased to call an examination, is an attempt, *per fas et nefas*, to raise up his table to the same level with mine.

4. I must now advert to a more disagreeable point. What Dr. Young calls his latest solution of the problem, published in the Philosophical Tranactions 1824, and another particular solution mentioned at the end of the XIVth Number of the Astronomical Collections in the Journal of Science, are both taken from my paper. First, it is certain that the solutions in question are particular cases of my general formulæ, when

a certain

a certain value is given to the indefinite index. Secondly, Dr. Young was aware that the two solutions were a part of my theory when he published them. For my paper was before the author in manuscript when he wrote the XIVth Number of the Astronomical Collections; and when he speaks of the equation $y = z^{\frac{3}{2}}$, the words imply that he had compared it with other particular cases of my general formulæ, having different exponents. The other formula, $y = \frac{3}{2} z^{\frac{3}{2}} - \frac{1}{2} z^2$, he never mentions without noticing that it belongs to a class of atmospheres which I had excluded; excluded, however, for no other reason than that they did not seem to approach so near nature as another class. Thirdly, Dr. Young has found out no physical property of the two atmospheres he has chosen which is not expressly developed in my paper. He lays, indeed, great stress on the mathematical property, which they possess, of exhibiting the refraction in a finite form. But, besides that this consideration is really of no moment and can weigh nothing in the balance against the least physical advantage, it is no discovery; for the two assumptions of Dr. Young are immediately deduced from my general formulæ, by choosing the particular value of the index that will make the radical in the expression of the refraction a quadratic trinomial. Fourthly, Dr. Young has nowhere investigated either of the two assumptions. It is easy to take the expressions when they are found out, and, by substituting them in the formula for the refraction, to show that the results are integrable; which is all that Dr. Young has done. But the question is how he came by them. Mere conjecture can hardly be supposed to lead to such particular formulæ. There must have been some train of thought which enabled him to choose two individual expressions in an infinite variety. But on this point we have no information. And we are as little informed on what grounds he asserts that the equation, $y = \frac{3}{2} z^{\frac{3}{2}} - \frac{1}{2} z^2$, belongs to an atmosphere which perfectly represents the true decrement of heat at the earth's surface. There is no proof of this in his paper in the Philosophical Transactions 1824, and he has nowhere else treated of the same formula. But every thing that I have mentioned he might find in my paper, which was in his hands, and to which he had paid some attention.

5. In the Philosophical Magazine for September 1821, I made some observations on Dr. Young's method for the refractions; and I shall now inquire whether, after so much dis-

 cussion,

cussion, what I then advanced requires to be qualified in any respect. Precision requires that we consider separately the infinite series, after the manner of Brook Taylor, and the finite formula which expresses the density by the four first powers of the refraction.

I made no observation on the infinite series, except that it does not converge; and I proved the want of convergency in a particular instance. This brought upon me a torrent of vehement writing not confined within the limits of *bienseance*. Since that time, mathematicians, whom Dr. Young dares not treat with so little ceremony, have come to the same conclusion. There is now so great a relaxation in asserting the convergency, that it is not necessary to add any thing more on this head.

In the Historical Sketch the finite formula of four terms is called an approximatory method. I cannot find any good reason why this name is given to it. As the coefficients are in some respects arbitrary, it certainly cannot approximate to any one determinate thing. But it may be thought to have a pre-eminent title to the appellation, because it will approximate to every thing. I called it empirical. This again exposed me to much violent writing, at the same time that the author, with an inconsistency not unusual to him, allowed that it was partly empirical. He now says that the coefficients are to be empirically modified. We are therefore likewise agreed on this head. I shall drop my offensive epithet, and use the phraseology of the author, which will equally serve my purpose.

The only explanation of the table of refractions in the Nautical Almanack that has ever been laid before the public, is the formula for calculation with its coefficients assigned in numbers. But in order to render this satisfactory, the author should at the same time have disclosed the considerations by which he determined what is arbitrary in the coefficients. Every astronomer can compute the table by means of the formula; but no one can construct the formula itself. There is no similar difficulty with regard to my table and the formula for its construction given in Dr. Young's paper in the Philosophical Transactions 1824. For here the table is known; and the empirical modifications mean nothing more than adjusting the coefficients so as to reproduce the numbers of the table. By the by, this making a table and a formula fit one another is very singular and curious, and may not unaptly be called playing at calculation. But in the other case, the table is the thing unknown; and we can attain a knowledge of it only by exploring

exploring the manner of its construction. What are the empirical modifications in this instance? Are they derived from original observations? Or must we seek for them in the tables already known to astronomers?

Entertaining a curiosity to understand something of the real nature of the table of refractions in the Nautical Almanack; and suspecting that original observations were not much employed; I thought of comparing it with the tables in the best repute at the time of its publication. I found that it perfectly coincided with M. Bessel's table in the *Fundamenta Astronomiæ*, as far as 45° from the zenith. Hence there is a great probability that the one table was used in the construction of the other. If there be 10 chances to 1 that two languages are in some way connected which have three names in common, as the author has determined in another place *, how great must be the chance of connexion in two short tables that perfectly agree in 45 consecutive numbers. At greater zenith-distances the refractions in the Nautical Almanack fall between M. Bessel's table and that of the French astronomers; and they ultimately become identical with the latter at the horizon. We can now form some idea of the nature of the empirical modifications of the formula. The table in the Nautical Almanack is a *melange* of the tables of M. Bessel and the French astronomers; and the numerical coefficients are adjusted accordingly. What I have now said is not to be considered as mere conjecture; it is the result of calculations that are easily verified.

The table of refractions in the Nautical Almanack is a singular instance of a set of practical calculations seemingly deduced from a deep theory; and yet, when we fully reach into the truth, having a slight, or even no, connexion with the theory. For it is not the formula which produces the table; it is the table, after the choice is fixed to make it consist of numbers taken from the tables of M. Bessel and the French astronomers, which determines the formula.

In finally retiring from so long and teasing a controversy, allow me, gentlemen, to thank you for the attention you have paid to the various papers I have addressed to you in the course of it. I remain, &c.

January 14, 1825. JAMES IVORY.

* Philosophical Transactions 1819, pp. 81, 82.

VII. *On*

VII. *On the Alteration of the Arcs of Vibration of Pendulums by the Hygrometrical Changes of the Air; and on a Compensating Pendulum of Deal, applicable to general Use.* By Tho. Squire, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SINCE the time of that eminent mathematician, astronomer and mechanic, Christian Huygens, great improvements have been made in the construction of horological instruments, and especially in those most important parts relating to the clock—I mean the scapement and the pendulum.

It is well known that our atmosphere is often subject to great and sudden vicissitudes with respect to pressure, temperature, dryness and moisture; and these causes, operating in a greater or less degree upon the different parts of the mechanism of the clock, must always have a tendency to render its motion unequal, and for that reason unfit for the purpose of showing the different portions of time with accuracy.

To obviate the effects arising from these different causes, many ingenious contrivances have been adopted.

The variations indicated by the barometer must in some degree affect the arc of vibration, by the changes of buoyancy and resistance; but the error from this source cannot be great, owing to the ponderous materials of which our best pendulums are made, their slow motion, smooth surface, and lenticular form.

On the other hand, the effects arising from the hygrometrical changes of the air do very perceptibly alter the arcs of vibration, they being considerably greater in a moist than in a dry state of the atmosphere; hence, the clock will lose in the former case and gain in the latter. This is a great evil, and I am of opinion that the most perfect jewelling of the pallets will not completely remove it.

In registering several hundred observations of the semi-arcs of vibration made by a clock having a dead scapement, going fusee work, a motive power of 2 pounds, and a pendulum of 12 pounds, I have often found these ares so variable, though mostly within narrow limits, as to defy calculation, leaving the mind in doubt whether to ascribe the variation in the *rate* to the arc of oscillation, or an imperfect compensation. The pendulum of this clock is so delicately hung, that a maintaining power at the pallets of no more than $\frac{2}{5}$ of a dram, including the friction of the train, is sufficient to keep it in motion.

I have

I have also found the variation in the arcs of vibration of a clock with a recoiling scapement, a heavy weight, and a light pendulum, to be still greater than in the one above mentioned, with a dead beat scapement, a light weight, and heavy pendulum.

From these cases it certainly appears reasonable to suppose that the *cycloidal checks* would be of service, and I see no good reason why they are not now used; as surely a model of so small a portion of the cycloid that would be necessary for the involute arc of vibration, performed by a seconds pendulum, could easily be made. But otherwise, I think Mr. Farnham's plan, of facing the pallets with segments of cylinders, to be the best that has ever been invented, for rendering the arc of vibration constant, provided that under such circumstances a perfect dead beat scapement can be preserved.

I come now to speak of the pendulum simply in itself; and from the nature and importance of this appendage to a clock it requires more than ordinary consideration. Now as it is well known that all metals expand with heat and contract with cold, for this reason the simple pendulum, when constructed wholly of such materials, is found to be useless where great accuracy is required. Accordingly, many ingenious compound pendulums have been invented, for the purpose of counteracting the effects arising from these causes, so that under every degree of atmospheric temperature the centre of oscillation may remain constant. Hence we have the angular, the lever, the conical, the gridiron, the mercurial, and a great many other compound pendulums: but I believe the last two are those mostly used in our best astronomical clocks; and if properly constructed, so that a just compensation is invariably preserved, they must be considered the best of the kind that have ever been invented. Valuable as these pendulums doubtless are in a philosophical point of view, yet they are found to be too expensive for general use. For this reason, I would recommend the wood pendulum in preference to all others, as combining in one important unity, cheapness and utility; and I am moreover convinced from experience, that a pendulum with a deal rod, previously prepared and properly managed, will perform, under similar circumstances, equally well as the best compound pendulum that has ever been invented.

For this purpose take a straight-grained well-seasoned piece of deal, perfectly free from knots, and which in the rough may be about one inch in breadth, half an inch in depth, and three feet and a half in length: let it be exposed for a considerable time to a gradual increasing heat, till at last its surface becomes a little charred; it may then be planed to its proper

breadth

breadth and thickness; and after cutting it to the required length, let the surface be well coated with copal varnish, and the ends dipped in melted sealing-wax, to prevent the least moisture penetrating the wood, which is of the utmost consequence to the accuracy of the pendulum in its simple state. But still, if, after all, the pendulum should be found to be in some degree affected by the changes of the atmosphere, we happily have in the nature of wood a remedy at hand for this inconvenience.

As the expansion and contraction of deal is much greater across than in the direction of the grain, cut a small block from the waste piece, equal in length to the width of the pendulum rod, and of the same thickness, which may be about ·78 of an inch by ·37 respectively; let this block be placed with its grain at right angles with that of the rod, having its lower side resting on the nut at the bottom of the pendulum, and its upper side supporting the bob, and which, if judiciously placed behind the front plate, will not only be nearly out of sight, but may be so contrived as equally to compensate for the upper part of the screw, and the small piece of spring on which the pendulum is hung. Experiment proves that with deal the block need not be more than half an inch in width for the compensation of a seconds pendulum rod of the same material.

It is almost unnecessary to remark, that the compensating block should be from a part of the same rod as the pendulum to which it is applied, and also be in every other respect similar as to previous preparation, &c.

I remain, gentlemen, yours, &c,

Epping, Dec. 15, 1824. THOMAS SQUIRE.

P.S. I take this opportunity of returning my best thanks to Dr. Burney and Mr. Veall, for their ready compliance with my request relative to the situation of their meteorological instruments.

I should like very much to see an account of some well conducted experiments on the expansion and contraction of wood, made under different degrees of temperature and dryness, with the ratio of the same across and in the direction of the grain; at the same time pointing out the best and most expeditious methods of entirely destroying this variable property of wood. T. S.

[We think it may be acceptable to our readers if we subjoin to the above communication with which Mr. Squire has favoured us, some extracts relating to the use of deal for pendulum rods from Mr. Baily's valuable Paper

on the Mercurial Compensation Pendulum, read May and June 1823 before the Astronomical Society, and just published in the second part of the Society's Memoirs, p. 385, and p. 411.—EDIT.]

On the Use of Wood for Pendulum Rods. By F. BAILY, *Esq.*

"Having enumerated the principal experiments that have been made on the expansion of mercury, I shall proceed to an examination of those which have been made, by various persons, on those substances of which the *rod* of the pendulum may be composed. Of all these substances, that of *wood* appears to be the least expansible; but, it is unfortunately so liable to be affected by the moisture of the atmosphere, that, notwithstanding we coat it with varnish, or sealing wax, or paint, or gilding, or even bake it, and impregnate it with oil, it has seldom been found sufficiently accurate for the refined purposes of the modern astronomer. I would not, however, wish to discourage any attempts to render this substance more fit for general use. Fitted up with a leaden bob (in the manner which I shall hereafter describe), it forms the cheapest pendulum that I know of: and, if placed in a room where there is an uniformity in the atmosphere, it might answer every useful purpose for an economical observatory. At all events, it would form an excellent appendage and improvement to the common household clock, and would be far superior to and much cheaper than the usual and absurd mode of hanging a leaden bob to the end of an iron wire.

* * * * * * * * *

"I have already stated that an economical pendulum for a seconds clock might be constructed by means of a leaden bob attached to a wooden rod: and I shall now show the mode of determining the relative lengths of these two substances. I have preferred lead to zinc on account of its inferior price, and the ease with which it may be formed into the required shape: and, as there is no considerable difference in their rates of expansion, it is equally applicable to our purpose. If we take the rate of expansion of deal to be ·0000022685, and that of lead to be ·0000159200, as given in the table, we shall have $\frac{\varepsilon}{\beta} = \frac{22685}{159200} = \cdot 1425 = k$. Now, if we assume the weight of the bob to be 100 times the weight of the rod (which will probably be the case), we shall by means of the equation (D) have $x = \cdot 2874\ (1 - \cdot 2926 + \cdot 3832) = \cdot 313$: which, being substituted for x in the equation (C), will give $r = 45\cdot 75$ and $b = 14\cdot 30$*.

* If the bob were made of zinc, the length of the cylinder would be only 13·88 inches.

This

"This being premised, let us assume $b=14\frac{1}{2}$ inches, and make the cylinder of any diameter, according to the weight proposed. The construction then of such a pendulum will be as follows. Take a deal rod, of a convenient size, but not less than 46 inches in length, the lower part of which ($14\frac{1}{2}$ inches long) should be made cylindrical, about $\frac{3}{8}$ of an inch in diameter: or the whole rod may be of this size and shape. Procure a leaden weight or cylinder to be cast, with a hole through the centre which will *freely* admit the cylindrical end of the rod, and of such a length that when put in the lathe it may be reduced to the required standard of 14·3 inches*, and to the required weight†.

"The lower end of the rod should be formed into a screw, to which a *wooden* nut may be fitted, in order to adjust the pendulum *nearly* to the given rate: and the final adjustment may be made by means of a slider, as above described‡. A pendulum of this kind will cost but a few shillings, and will answer many useful purposes, as I have found by experience. If the expansion of the rod could be depended on, it would be as accurate as any other. In this construction I have not considered the expansion of that part of the *spring* which is below the axis of motion. The effect of this may be taken into account, at the final adjustment of the pendulum.

"In all the wooden pendulums which I have seen, the bob has been made of a *lenticular* shape, and oftentimes been fastened to the rod by means of *a pin passed through its centre*; and therefore not constructed with any view to compensation. This shape was, I presume, originally adopted with a view to overcome the *resistance of the air*, and thus reduce the maintaining power of the clock. But it is well known that the air, as a resisting medium, has no sensible effect on the rate of the clock; neither is the rate affected by the shape of the bob; we may therefore choose that shape which will best answer other useful purposes."

* *It will be more convenient to have this cylinder too long rather than* too short: since we may readily diminish the length of it, if, on trial, it should be found that the pendulum is *over*-compensated.

† The following are the respective weights of a leaden cylinder 14·3 inches long, and having a hole, through the centre, equal to ⅜ of an inch in diameter.

Diameter of Cylinder.		Weight.
1¼ inch.	=	6·56 lbs.
1½	=	9·73
1¾	=	13·47
2	=	17·80
2¼	=	22·70

‡ A small sliding weight attached to the rod, as described in p. 402 of the Memoirs.

VIII. *On*

VIII. *On the Insect called* Oistros *by the Ancient Greeks, and* Asilus *by the Romans. By* William Sharp MacLeay, *Esq., M.A., F.L.S.**

THE determination of the animals and plants mentioned by the ancient writers must always be a pleasing subject of research, tending, as it does, not merely to our better comprehension of the meaning of these authors, but also to our better acquaintance with the mysteries of nature. Every classical reader, as well as every entomologist, is familiar with the word *Oestrus* as the name of one of the most celebrated insects of antiquity. The insect itself, however,

> "cui nomen *Asilo*
> Romanum est, *Œstron* Graii vertere vocantes."
> Virg. Georg. iii. 147.

has not for this been the more accurately determined; and Olivier is the first modern naturalist who appears to have suspected that the *Œstrus* of the ancients and the *Œstrus* of the moderns are totally different insects. With an exception in favour of Messrs. Latreille, Kirby and Spence, this curious remark seems not to have excited much attention; although it may easily be proved that Olivier has come much nearer the truth than those who hold the contrary opinion.

In investigations of the following nature, it is not only advantageous but necessary to begin from some fixed and indisputable position. Now such I take to be the identity of the insects termed in French *taon;* in Spanish *tavano;* in Italian *tabano;* and in Latin *tabanus.* The *tabani* are unfortunately insects too common for their name to have ever been forgotten; and knowing what the country-people in France call *taons*, we know the insects which Pliny anciently termed *tabani.* By comparing Pliny with Aristotle, we find that he invariably translates the word μύωψ (*cæcutiens*) by the Latin word *tabanus;* and entomologists know well that this Greek name is extremely appropriate to the modern *tabani* or *taons*, which are so remarkable for their eyes, that a common species of *Chrysops* has at the present day the trivial epithet of *cæcutiens.* Now it appears from Aristotle, that the οἶστρος† and μύωψ were insects extremely near each other in affinity; they are almost always mentioned by him together, and agree in every respect but that wherein Aristotle was least likely to be accurate,

* From the Transactions of the Linnean Society, vol. xiv. p. 353.

† Οἶστρος is a name also applied by Aristotle to some small insectivorous bird, and to some species of the *Cymothoadæ*, which is parasitical about the fins of the Tunny. Pliny also appears to apply the word *Œstrus* to the drone (lib. ii. c. 16.).

rate, namely, their mode of generation. In description they always accord; they are both *diptera*, and therefore he says necessarily ἐμπροσθόκεντρα, "οὐδὲν δ' ἔστι δίπτερον ὀπισθόκεντρον." Now this, by the way, proves no tonly that the οἶστρος was not the modern *Œstrus*, but moreover that Aristotle could never have seen a modern *Œstrus* attack cattle; for had he seen it, he would most assuredly have deemed it ὀπισθόκεντρος. And yet he must have seen his οἶστρος about cattle; for he states positively not only that the οἶστροι pierce the hides of quadrupeds, but that they are armed with a strong tongue, and are blood-suckers (αἱμοβόρα ζῶα). In both these last respects it is to be observed, that they differ totally from the modern *Œstrus*, but perfectly agree, as M. Latreille has well said, with the Linnæan *Tabani*.

Ælian describes the οἶστρος and μύωψ in the same way as Aristotle. They are both most inimical to cattle (βουσὶν ἔχθιστα). The οἶστρος he states to be one of the largest flies (κατὰ τὰς μυῖας τὰς μεγίστας), having a strong sting in its mouth, and uttering a particular kind of harsh humming noise (ἦχον βομβώδη τινὰ καὶ τραχύν). The μύωψ, on the other hand, he says is like the fly called by the Greeks κυνόμυια; and although it makes a louder hum than the οἶστρος, he states that it has a smaller sting.

If we now turn to the poets, we shall find that their account of this insect tallies perfectly with the above description of the ancient naturalists, but not at all with the modern genus *Œstrus*.

Homer describes his *Œstrus* as αἰόλος, a word which applies admirably to the most common of all *Tabanidæ*, namely the *Tabanus pluvialis* of Linnæus, as well as to the insects which now form the genus *Chrysops*. And the Scholiast, after stating that the οἶστρος and μύωψ are very near in affinity, says that the latter differs in having a smaller sting in the mouth, and in being subæneous in respect to its aspect or *facies* (ὑπόχαλκον τὴν μορφὴν), thus evidently pointing, as I think, to the difference which exists between the modern genera *Tabanus* and *Hæmatopota*, the latter having much more splendid eyes. That Homer's insect was not the modern *Œstrus* may besides be inferred from what he says of the season in which it makes its appearance,

"Ὥρῃ ἐν εἰαρινῇ, ὅτε τ' ἤματα μακρὰ πέλονται"

for there are few cases, I believe, of the modern *Œstri* appearing earlier than the middle of July. And this circumstance, by the way, leads also to the conclusion, that the English *breese* or *brize* is not the modern *Œstrus*, although it is generally

rally understood so to signify in the following punning lines of Shakespeare:

> " Cleopatra,
> The *breeze* upon her, like a cow in June,
> Hoists sail and flies."

Now Mouffet, who, both as an entomological observer and as a contemporary of Shakespeare, was likely to know the insect then named *brize*, says expressly that the *breeze*, *clegg*, *clingez* and *taon*, are all the same insect, his description of which proves it to be no other than the *Hæmatopota pluvialis*, for which the *Clegg* remains to this day the well-known and appropriate provincial name—a name totally inapplicable to the modern *Œstrus*.

I have before said, that Aristotle makes it quite evident that his οἶστρος and μύωψ were very nearly of the same construction. So near indeed in affinity do they appear to have been, that Æschylus would seem to consider them as identical in his *Prometheus vinctus*. From this poet we learn, that they are ὀξύστομοι, and pierce the skin. Io says,

> " Οἰστρηλάτῳ δὲ δείματι δειλαίαν
> Παράκοπον ὧδε τείρεις;"

In short, wherever the μύωψ is distinguished from the οἶστρος, I take the former to be either a *Chrysops* or *Hæmatopota**, or some insect near to them, and the latter to be some species of the modern genus *Tabanus*, probably the *Tabanus bovinus* Linn. or dun-fly, whose power of agitating cattle I have myself had occasion to witness. This last insect certainly appears to be the *Asilus* and *Œstrus* of Virgil. That this poet's insect cannot be identical with any modern *Œstrus* is clear from his describing it to be in great plenty, and to be " acerba sonans." Now the *Œstrus bovis* is very rare every where; and, according to Mr. B. Clark, makes no noise. The *Œstrus equi* is also silent in flying, as I have repeatedly myself observed. So that neither of these insects can be that which is celebrated by Virgil, whose description of the ability of the ancient οἶστρος to make a particular kind of humming noise is corroborated by the Scholiast before mentioned as well as by Ælian.

Messrs. Kirby and Spence in their Introduction to Entomology think that the ancient *Myops* was some species of Latreille's genus *Tabanus*, and that the *Œstrus* of the Greeks may either have been a *Pangonia* or a *Nemestrina*. What we

* One circumstance which is mentioned by Ælian respecting the *Myops*, namely, that it makes a louder hum than the *Œstrus*, is perhaps against its identity with the modern genus *Hæmatopota*.

know,

know, however, of the latter genus answers in no one respect to the description above given of the ancient *Œstrus*, which certainly was an insect allied to the modern *Tabanus*; whereas *Nemestrina* has no immediate connexion with it either in economy or structure. Besides, no *Nemestrina* has ever yet been found in Europe. The argument for *Pangonia* is rather stronger, as this is not only an European genus, but one nearly allied to *Tabanus*. Aristotle however says, that his *Œstrus* and *Myops* have both a strong tongue (ἰσχυρὰν γλῶτταν ἔχουσι); a description in perfect accord with the mouth of a modern *Tabanus*, but quite at variance with the long, weak and flexible proboscis of *Pangonia*, which can scarcely be supposed capable of piercing the hide of an ox. Olivier and Latreille indeed both state, that the long trunk of *Pangonia*, like that of *Bombylius*, only serves for sucking flowers. But to insects that suck flowers Aristotle expressly places his οἶστρος in opposition.

It is rather interesting to remark the manner in which the early modern naturalists viewed this subject. Mouffet's opinion is, as far as I can make it out, the same with mine given above. At all events he considers the μύωψ of the Greeks to be our *Hæmatopota pluvialis*. Ray, on the other hand, considers this insect to be the οἶστρος, as we may judge from the following description, "Musca bipennis Œstrum dicta, alis membranaceis punctis crebris nigrioribus velut adspersis:" which is clearly the *Hæmatopota*.

Valisnieri appears to have been the first naturalist of any repute who took the modern *Œstrus* to be that of Virgil, while Martyn and other commentators seem to have adopted his opinion. The first insect, which Linnæus considered to be the *Œstrus* of the ancients, appears to have been a species of the modern genus *Asilus*, probably the *Asilus crabroniformis*, as we learn from his *Lachesis Lapponica*. This was a gross error; and he soon rectified it, as he thought, by adopting the opinion of Valisnieri. It is not indeed unlikely that some of the ancients* should, like Valisnieri, have seen the perfect insects of the modern *Œstrus* flying about cattle, and that they should have witnessed the extraordinary agitation which they produce:

* Aristotle was not certainly one of these ancients; for he could never have seen a female of the modern *Œstrus*, as appears from his stating that no dipterous insect has its sting placed behind. It seems however to have escaped the notice of naturalists, that this great philosopher was acquainted with, and has described the larva of one of the modern family of *Œstridæ*; and, as is rather singular, precisely that larva which Reaumur describes as infesting the fauces of the stag, but of which the perfect insect remains still unascertained.—See *Arist. Hist. Anim. lib.* ii. *c.* 18; and *Reaum. tom.* v. 67—77.

produce: but however this may be, they certainly appear to have always confounded such insects with the more common *Tabani*; for it is the modern *Tabanus*, or some genus extremely near to it, that they have always described as the οἶστρος.

I shall take this opportunity of quoting a passage from Mouffet, which proves that he was acquainted with the modern genus *Œstrus*, although he did not confound it with the ancient οἶστρος. The passage will also show us how valuable is the information sometimes to be procured from this obsolete work; since, if we connect it with what Reaumur has said of the *Œstrus equi*, we have almost the whole economy of this interesting insect:

"His proximè accedit alia musca bobus et jumentis interdiu sole fervido infesta, quam Pennius *Curvicaudam* sive σκολιουρὸν jure appellat. Semper enim cruribus aut ventri jumenti insidens, caudam versus ipsam recurvam tenet et spiculum exertum quo ad percutiendum cauda sit paratior (δίπτερον ὀπισθόκεντρον). Hanc Angli *a Whame and a Burrell-flye* proprie vocant, nec nisi in Angliâ facile invenitur. Musca hæc api fere similis formâ et colore, sed corpore est crassiore. Non adhæret nec sanguinem sugit sed solummodo stimulo in caudâ pungit, atque ut equos affligat per longissima itinera ipsos volando persequitur. Equi naturâ hanc muscam timent et ad ejus solum contactum quasi horrent, caudâ pedibusque et labiis tam cruentum hostem abigere sæpe conantes. Sunt qui putant hanc muscam non aculeo pungere, sed stercora (*ova*) pilis equi affigere caudâ, unde posteà molestissimæ lendes gignuntur. Magno quidem impetu sed cæco ad prædam *Tabanus* atque Σκολιουρὸς feruntur." *p.* 62.

IX. *On a supposed Anomaly in the Rotation produced by the Galvanic Current. By Mr.* W. STURGEON.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IT appears to have been hitherto remarked by every writer on Electro-magnetism, that the direction of rotation is always reversed by changing the direction of the Galvanic current, provided the magnetic bar remains unmolested. It has so happened, however, in one of my experiments, whilst rotating a galvanic wire round the pole of a magnetic bar, that, although the latter be not altered in its position, yet the former will continue to rotate around it in the same direction, whatever

ever change be made with respect to contact; that is, whether the wire be descending from the zinc, or descending from the copper side of the battery.

Now as this phænomenon appears somewhat anomalous to every other electro-magnetic experiment yet made public, I should feel obliged if any of your scientific correspondents would make known through the medium of your valuable and widely disseminated Journal, if a like phænomenon has ever occurred during their experiments; or, according to the present received doctrine of electro-magnetism, under what circumstances this invariable rotation can possibly happen.

Your most obedient servant,

W. Sturgeon.

Artillery Place, Woolwich,
Jan. 21st, 1825.

IX. *Notices respecting New Books.*

The Natural History of the Bible; or A Description of all the Quadrupeds, Birds, Fishes, Reptiles, and Insects, Trees, Plants, Flowers, Gums, and Precious Stones, mentioned in the Sacred Scriptures. Collected from the best Authorities, and alphabetically arranged. By Thaddeus Mason Harris, D.D. Boston, New England. Wells and Lilly, 1820, 8vo, pp. 476. Reprinted by T. Tegg, London, 1824.

AMONG the valuable contributions to science and literature with which our American brethren are now enriching our language, we are happy to notice this useful volume. The want of such a work has been much felt in this country; and we understood some time since that both Dr. Leach and the Rev. C. Wellbeloved meditated such an undertaking. It is of essential service to the public to possess works on subjects of common interest, comprising in a small compass what before could not be found without access to voluminous authors and extensive libraries. The work before us is limited in its object, and without being prolix is generally sufficiently full. We know not of any other book on the same plan; and it is not rendered superfluous by late translations of separate books of the Sacred Writings, or by running commentaries on the same.

In order fully to understand the Sacred Writings, a knowledge of whatever is local and peculiar becomes important. Not the least important, as contributing to the illustration of Scripture, is Natural History. The poetical books of the Hebrews, in particular, abound in lively comparisons, local allusions, and strong metaphors, drawn from material objects, whose

whose most powerful charms arise from their individuality. The real import of the sentiment, expressed by such allusions and metaphors, must be gathered from a knowledge of the objects on which they are founded. Much of the poetry of the Hebrews, like that of every people of a remote age, partakes largely of the pastoral kind, resulting from the personal occupation of the authors, or the common condition of mankind. To enjoy the beauty of the pastoral scenery which is so often alluded to in the Hebrew Scriptures, one should have some knowledge of the climate and natural productions of the country which furnishes it; and every thing which tends to make the sacred Scriptures more engaging to the mass of readers, by illustrating what is obscure, is a great good.

In regard to the Botanical part, Dr. Harris names, as his primary authorities, Hiller* and Celsius, the latter of whom is spoken of with great respect by Linnæus, as the most consummate *Polyhistor* of his age. Though this friend and patron of Linnæus devoted a great part of a long life to the illustration of the plants mentioned in the Scriptures, yet he did it under so many disadvantages, that Linnæus, in the interval between the publication of the first and second volumes of the *Hierobotanicon* of Celsius, (1745—1752,) mentioned a *Flora Palæstina* as among the *desiderata*; and declared that whoever should visit the Holy Land, and make a collection of the plants of Palestine, would be immortalized by theologians. Stimulated by these remarks, which fell from Linnæus in one of his lectures, Hasselquist, then a student of medicine, bent all his efforts to the accomplishment of the great and difficult undertaking. Having already made great advances in Natural History, he studied the Arabic language, and with much difficulty and delay procured scanty pecuniary means for his expedition. When he arrived at Smyrna, says Linnæus, he was treated with the utmost hospitality by the Consul-general, who sent him to Egypt; and having remained at Cairo about a year, he pursued his travels through Arabia and Palestine, diligently collecting all the plants he could find, and describing the animals and stones which he met with. After his return to Smyrna, he died of the disease under which he had long laboured, and his creditors took possession of his manuscripts and collections. The queen of Sweden redeemed them, and directed Linnæus to arrange and publish the writings of Hasselquist, at the same time giving to him specimens of all the plants of which she had duplicates. Of these Linnæus gave an account in his *Flora Palæstina*, to which he added a few that were collected by Pococke, Rauwolf, and Shaw.

* *Hierophyticon*, 4to, 1725.

Bruce,

Bruce, whose reputation as an authority has, contrary to that of some travellers, increased with the increasing knowledge obtained of the countries which he visited, contributed considerably to the stock of information concerning the Natural History of the Bible. Preparatory to his great expedition, he studied the Oriental languages, at Algiers, with great zeal and diligence; and from a knowledge of the original languages of the Scriptures, he claims an advantage over previous travellers in the East, who were either not at all, or but very superficially acquainted with those languages. He made it a rule also, in describing plants and animals which he saw, to prefer those mentioned in Scripture, particularly where doubts had arisen among translators and commentators. To these authorities on the subject of plants, Dr. Harris adds Dioscorides and Pliny, among the ancients; and Alpinus, Rauwolf, Shaw, Russell, Forskal, and others, among the moderns.

The author's leading authority concerning the Animals mentioned in Scripture is Bochart, a learned orientalist of the seventeenth century. His *Hierozoicon*, which, as its name imports, relates to the animals spoken of in the sacred writings, was printed at London in 1663. Great accessions have been made to this department of knowledge since that period, giving certainty to what was doubtful, and correcting what was erroneous; yet it seems, for the most part, that the opinions of this indefatigable scholar are confirmed by the testimony of the most learned and intelligent travellers since the period in which he wrote. Dr. Harris, though one might apprehend from his preface that he had relied too unhesitatingly on Bochart, is not wanting in the examination of subsequent authorities*, and giving them their due weight in coming to his own decisions.

Another

* We may cite as an instance the following passage from the article Lily, שושן (p. 237): " Mr. Salt, in his Voyage to Abyssinia, p. 419, says, 'At a few miles from Adowa, we discovered a new and beautiful species of *Amaryllis*, which bore from ten to twelve spikes of bloom on each stem, as large as those of the *Belladonna*, springing from one common receptacle. The general colour of the corolla was white, and every petal was marked with a single streak of bright purple down the middle. The flower was sweet-scented, and its smell, though much more powerful, resembled that of the lily of the valley. This superb plant excited the admiration of the whole party; and it brought immediately to my recollection the beautiful comparison used on a particular occasion by our Saviour, I say unto you that Solomon in all his glory was not arrayed like one of these.' And Sir J. E. Smith (Considerations respecting Cambridge), observes, 'It is natural to presume the divine teacher, according to his usual custom, called the attention of his hearers to some object at hand; and as the fields of the Levant are overrun with the *Amaryllis lutea*, whose golden liliaceous flowers

Another authority, confined to no particular department of the Natural History of the Bible, is J. J. Scheuchzer, who died in 1731. His great work, *Physique Sacrée*, embraces a wide range of inquiry and speculation, not only concerning the Natural History of the Bible, but every thing remarkable in the works of art. It may be supposed, that in eight folio volumes on the subjects above enumerated, there would be enough, and more than enough, both of that which depends on evidence, and of that which is merely theoretical; but to an author who knows how to separate the wheat from the chaff, superfluity, though often troublesome, does not produce loathing or disgust. It was, we presume, by such a spirit of patient inquiry, that Dr. Harris was enabled to endure the strange vagaries of Parkhurst, for the sake of what so fearless a theorist might sometimes, even by his boldness, contribute towards probability and truth. Of Scheuchzer Dr. Harris has made principal use for determining the serpents and insects mentioned in Scripture. Rudbeck is his principal authority for the fishes, and Lemnius and Braunius for the minerals and precious stones.

In the use he has made of the authors above mentioned, and of various others, Dr. Harris manifests a due discrimination, and puts it in the reader's power generally, in cases of doubt, to weigh the evidence for himself: and we consider him to be entitled to the thanks of the public for having brought within a reasonable compass the most valuable materials on the subjects of which he treats; for having arranged them in a convenient method; and, in general, for having arrived at his own conclusions, on the best evidence which the subjects admit.

A Correct Abstract of the New Act for ascertaining and establishing Uniformity of Weights and Measures: to which are added Six Original Tables, comprising the Old and New Standards, &c. &c.; *by the aid of which Dealers and Purchasers may easily understand the total Change throughout the United Kingdom, which will take place May* 1st, 1825. By Henry Butter, Master of the Academy, Goswell Road. London, 1825. 12mo pp. 40.

We are glad to see the class of cheap publications familiarly explaining and reducing to the purposes of business the

flowers in autumn afford one of the most brilliant and gorgeous objects in nature, the expression of Solomon in all his glory not being arrayed like one of these, is peculiarly appropriate. I consider the feeling with which this was expressed as the highest honour ever done to the study of plants; and if my botanical conjecture be right, we learn a chronological fact respecting the season of the year when the Sermon on the Mount was delivered.'"

 provisions

provisions of so important a public measure, as the Weights and Measures Act, commenced by Mr. Butter's pertinent and correct little work: the contents are so fully stated in the title, that it is not requisite to detail them; and as their nature affords no scope for criticism or quotation, we may dismiss the subject with this general opinion of their utility.

ANALYSIS OF PERIODICAL WORKS ON NATURAL HISTORY.

Curtis's Botanical Magazine. No. 454.

Pl. 2523. *Calceolaria rugosa*, a native of Chili;—this and *integrifolia* the Editor considers as only varieties.—*Ageratum mexicanum*, "hispidum, foliis cordato-ovatis crenatis rugosis, corymbo composito, paleis pappi lanceolatis aristatis;"—from seeds brought from Mexico by Mr. Bullock.—*Limnocharis Plumieri.*—*Heliophila stricta*, "caule stricto, foliis pinnato-dentatis integrisque hirsutis, siliquis linearibus subtorulosis pubescentibus erectis clavato-mucronatis." This species agrees in many respects with *coronopifolia*, and must be near to *pilosa* in DeCandolle's system.—*Melodinus monogynus* from the East Indies.—*Iris longispatha*, "imberbis, foliis lineari-lanceolatis falcatis scapo subtereti tortuoso, germinibus dodecagonis, spathâ exteriori longissimè antenuatâ." Introduced in the Chelsea Garden last year from Russia by Dr. Fischer.—*Cynoglossum nitidum.* Native of Portugal. This has been recently and needlessly placed in a separate genus under the names of *Omphalodes* and *Picotia.*—*Jussieua ovalifolia*, "caule erecto ramoso, ramis tetragonis subulatis foliis ellipticis acuminatis nervoso-venosis villosis, calycibus tetraphyllis ovatis acuminatis trinerviis hirtis." Raised from seeds from Madagascar.

The Botanical Register. No. 217.

Pl. 840. *Catasetum Claveringi*, "spicâ foliis breviore, labello carnoso apice tridentato, sepalis oblongis obtusis: interioribus maculatis." Brought from Brazil in 1822 by Mr. George Don. "It is a far more remarkable plant," observes Mr. Lindley, "than the *C. tridentatum* of Hooker, and altogether the most singular Orchideous plant which has yet been seen in a cultivated state." A sketch of the history of this remarkable genus is given during its first year "in which period alone it has increased from one certain and one uncertain species to five certain and one uncertain species: Div. I. 1. *maculatum*, 2. *tridentatum*, 3. *Claveringi*, 4. *Hookeri*, 5? *macrocarpum*: Div. II. 6. *cristatum.*—*Dracocephalum nutans*, a hardy perennial from the Altai mountains.—*Boronia serrulata*, a very beautiful plant from New Holland.—*Acacia undulata;* a green-house plant native of New Holland, flowering in winter.—*Camaridium ochroleucum;* an orchideous plant from Trinidad, where it appears to be parasitical on the trunks of trees; not hitherto described.—*Reaumuria hypericoides.*—*Coreopsis tinctoria*, discovered by Mr. Nuttall in the Arkansa country, North America.

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 13.—A PAPER by Capt. H. Kater, was read, entitled A Description of a Floating Collimator.

This instrument is destined to supply the place of a level or plumb-line in astronomical observations, and to furnish a ready

ready and perfectly exact method of determining the position of the horizontal or zenith point on the limb of a circle or zenith sector. Its principle is the invariability with respect to the horizon of the position assumed by any body of invariable figure and weight floating on a fluid. It consists of a rectangular box containing mercury, on which is floated a mass of cast-iron about twelve inches long, four broad, and half an inch thick, having two short uprights or Y's of equal height, cast in one piece with the rest. On these is firmly attached a small telescope furnished with cross wires, or, what is better, crossed portions of the fine balance spring of a watch, set flat-ways, and adjusted *very exactly* in the sidereal focus of its object-glass. The float is browned with nitric acid to prevent the adhesion of the mercury, and is prevented from moving laterally by two smoothly polished iron pins, projecting from its sides in the middle of its length, which play freely in vertical grooves of polished iron in the sides of the box. When this instrument is used, it is placed at a short distance from the circle whose horizontal point is to be ascertained, on either side (suppose the north) of its centre, and the telescopes of the circle and of the collimator are so adjusted as to look mutually at each other's cross wires (in the manner lately practised by Messrs. Gauss and Bessel), first of all coarsely, by trial, applying the eye to the eye-glasses of the two instruments alternately, and finally by illuminating the cross wires of the collimator with a lanthorn and oiled paper, (taking care to exclude false light by a black screen having an apperture equal to that of the collimator,) and making the coincidence in the manner of an astronomical observation, by the fine motion of the circle. The microscopes on the limb are then read off, and thus the apparent zenith distance of the collimating point (intersection of the wires) is found. The collimator is then transferred to the other (south) side of the circle, and a corresponding observation made *without reversing the circle*, but merely by the motion of the telescope on the limb. The difference of the two zenith distances so read off is double the error of the zenith or horizontal point of the graduation, and their semi sum is the true zenith distance of the collimating point, or the co-inclination of the axis of the collimating telescope to the horizon.

By the experiments detailed in Capt. Kater's paper, it appears that the error to be feared in the determination of the horizontal point by this instrument, can rarely amount to half a second if a mean of four or five observations be taken. In a hundred and fifty one single trials, two only gave an error of two seconds, and one of these was made with a wooden float.

In

In upwards of a hundred and twenty of these observations, the error was not one second.

For further details we must refer to the original communication.

Jan. 20.—A paper on some improvements in the construction of the barometer, by J. F. Daniell, Esq. F.R.S. was read.

Jan. 27.—A paper was read, on the anatomy of the mole cricket; by John Kidd, M.D. F.R.S.

LINNÆAN SOCIETY.

Jan. 18.—A further portion of the Rev. Messrs. Sheppard and Whitear's catalogue of Norfolk and Suffolk birds was read.

ASTRONOMICAL SOCIETY.

Jan. 14.—At the meeting this evening, Mr. Baily laid on the table, for the inspection of the members, two micrometers, which have been recently invented and constructed by M. Frauenhofer, of Munich*.

These micrometers are formed by means of very fine lines cut on glass with a diamond point in a peculiar manner; and placed in the focus of the telescope. One of these micrometers consists of concentric circular lines drawn at unequal distances from each other; and the other consists of straight lines crossing each other at a given angle. The mode of cutting these lines has furnished M. Frauenhofer with a method of illuminating them, which (at the same time that it renders the lines visible) leaves the other part of the field of the telescope in darkness: so that the transits of the smallest stars may be observed by means of these micrometers; the lines appearing like so many silver threads suspended in the heavens. A short account of the circumstances which led M. Frauenhofer to this happy invention was read.

An engraving of Frauenhofer's achromatic telescope, now at Dorpat, of 14 feet focus and 9 inches aperture, was also submitted to the inspection of the members present, by Mr. Herschel.

A communication was read from Capt. Ross, dated Stranraer, 7th August 1824, in which he transmits a diagram exhibiting his observation of the occultation of Herschel's planet by the moon, on the preceding day, with Ramage's 25-feet telescope, and a power of 500. The planet appeared to have entered about one-third of its diameter on the dark part of the moon before it disappeared, and its light began to diminish before it touched the lunar disc. On the contrary, at its emersion it appeared one-fourth of its own diameter distant

* See Phil. Mag. vol. lxiv. p. 210.

from

from the moon's western limb. The whole time of the occultation was $1^h\ 7^m\ 44^s{\cdot}5$.

After this, the reading was commenced of a paper by Mr. Henry Atkinson, of Newcastle-upon-Tyne, "On astronomical and other refractions; with a connected inquiry into the law of temperature in different latitudes and altitudes." As the reading of this paper will be resumed at a subsequent meeting, an abstract of the whole may with propriety be deferred.

GEOLOGICAL SOCIETY.

Jan. 21.—A paper was concluded, entitled "On a recent formation of freshwater rock marl in Scotland, with remarks on shell marl, and on the analogy between the ancient and modern freshwater formations." By Charles Lyell, Esq. Sec. G. S.

The rock marl described in this communication is an extremely compact limestone, in part of a crystalline structure, and traversed by numerous irregular tubes or cavities.

As a principal part of its geological interest is derived from its recent origin, the author has drawn a brief sketch of the physical structure of the county of Forfar, in order to explain distinctly its position.

Those strata are also enumerated in which limestone is found, and its remarkable scarcity in Forfarshire pointed out.

The districts to which shell marl is confined are next considered, and it appears that deposits of this nature are accumulated only in lakes in two formations; viz. the inferior or transition sandstone, and the old red sandstone.

The Bakie loch, in which the rock marl occurs, lies in a hollow in sand and gravel. This gravel consists of the broken and rounded masses of the primitive rocks of the Grampians, which are heaped in large quantities upon the old red sandstone in the valley of Strathmore.

The succession of the deposits of sand, shell marl, and rock marl, in the lake of the Bakie now drained, is then described. The shells and plants inclosed in the rock are the same as those in the soft shell marl, and are all still living in the waters on the spot. Among the plants are the stems and seed-vessels of *Charæ*, the latter being fossilized in such a manner as to present a perfect analogy to the gyrognite of the ancient freshwater formations.

Mr. Lyell then considers the probable origin of the rock marl, which appears to be derived from subjacent shell marl, through which springs ascend, charged with carbonic acid.

Some remarks are next offered on the shell marl of Forfarshire, and some which the author has examined near Romsey in

in Hampshire is described. The subjects of chief interest with regard to the shell marl are, its slow growth, the small proportion of full-grown shells which are found in it in Forfarshire, the greater rapidity of its growth in the vicinity of springs, its abundance in a part of Scotland in which limestone is very rare, and its scarcity in the calcareous districts of England.

The question is then considered, whether the shell marl be exclusively derived from the exuviæ of testacea, and the various arguments for and against this hypothesis are entered into.

In conclusion, Mr. Lyell takes a general view of the analogy between the ancient and modern freshwater formations.

Both of these may be described generally as consisting of thin beds of calcareous, argillaceous, and arenaceous marls, together with strata of sand and clay, to which the consolidated beds bear upon the whole but a small proportion.

The shells and plants contained in both are referable to the same genera.

The bones and skeletons of quadrupeds are found buried at various depths in the marls of Forfarshire, as they occur in the lower freshwater formation of Paris.

Of the four desiderata mentioned by Messrs. Cuvier and Brongniart (Essay on the Environs of Paris, p. 56), as being requisite to complete the analogy between the deposits of lakes now existing and those of a former world, three are supplied by the lakes in Forfarshire: viz. 1. a compact limestone; 2. vegetables converted into the substance of their calcareous matrix; 3. large beds of yellowish white calcareous marl.

The rock marl of Forfarshire closely resembles the Travestino of Italy, part of which is a recent formation, but part has been proved by M. Brongniart to be of a date probably as ancient as the upper freshwater strata at Paris.

The only difference remaining between the ancient and the modern freshwater formations is, 1. the absence in the latter of silex, which is only known as a modern deposit from water connected with volcanic agency; and 2. the small scale on which the recent accumulations proceed.

If these differences are ascribable to a higher temperature prevailing where the ancient freshwater rocks were formed, they may perhaps disappear when the hitherto unexplored tropical regions of the globe are fully investigated.

MEDICO-BOTANICAL SOCIETY.

At a meeting of this Society holden on Friday the 14th instant, the Professor delivered a lecture upon a new essential oil lately introduced from South America. It is called the Essen-

sential Oil of Laurel, which is not only an unsatisfactory name, but it is also incorrect, as such might convey an idea that it is obtained from some of the Laurels which are of the genus *Prunus*, while the plant from which this oil is extracted is of the genus *Laurus;* but still it does not appear from what species of this genus. It is extracted from the inner bark or *liber* of the plant, which is effected through a particular process, and must be performed by persons accustomed to the practice.

Neither its chemical components nor its medicinal properties have been yet ascertained. It is certainly of a very volatile nature, as well as aromatic. The Indians hold it in high estimation for its medicinal properties, using it in various cases; applying it sometimes internally, and at others externally. The high encomiums they pass upon its medicinal properties will scarcely obtain credit, though it is probable it will in some instances prove serviceable. The Professor will at the next meeting resume his Lecture upon this oil, when he intends to make some chemical experiments upon it.

The anniversary meeting of the Society was held on Monday the 17th instant, when the following officers were elected for the ensuing year: *President*, Robert Bree, M.D. F.R.S.—*Vice-Presidents*, John Ayrton Paris, M.D. F.R.S.; Edward Thomas Monro, M.D.; Joshua Brookes, Esq. F.R.S.; William Thomas Brande, Esq. *Sec.*R.S.; Sir James M'Gregor, M.D. F.R.S.; Sir Alexander Crichton, M.D. F.R.S.—*Director*, John Frost, Esq.—*Treasurer*, William Newman, Esq. —*Secretary*, Richard Morris, Esq.—*Honorary Librarian*, Dr. Edward Thomas Monro, (V.P.)—*Professor of Botany*, John Frost, Esq. (Director).—*Curator of the Collection*, Richard Morris, Esq. (Secretary).—*Council*, The President, Vice-Presidents, and other Officers; together with Dr. John Elliotson; Thomas Jones, Esq.; William Yarrell, Esq.; Thomas Gibbs, Esq.; Henry Tatham, jun. Esq.

ROYAL INSTITUTION OF CORNWALL.

At the sixth annual meeting of this Association, held at Truro, August 27, 1824, Sir C. Hawkins, Bart. M.P., in the chair, the subjoined Report of the Council was ordered to be printed, and the following Resolutions were passed, with others of merely local importance.

"That the Right Hon. Edward Viscount Exmouth be re-elected President of this Society.

"That J. H. Vivian, H. Willyams, J. Williams, jun., T. Daniell, and W. Paul, Esqrs., be elected Vice-Presidents for the ensuing year, and with the following members form the Council;—

cil:—Dr. Taunton, Captain Forster, Mr. Chilcott, Mr. Carpenter, Mr. Turner, Mr. S. Moyle.

"*Secretaries*—Mr. W. M. Tweedy and Mr. J. T. Nankivell.

"That Dr. Potts be re-elected Lecturer on Chemistry and Experimental Philosophy."

Report.—The Council of the Royal Institution of Cornwall have the pleasure to report to the Sixth Annual Meeting, that the Donations to the Museum during the past year have, in number and importance, equalled those in any year since the first, and prove that the Society is looked on with a favourable eye even by those not immediately interested in its prosperity.

The state of the Museum will, your Council believe, fully satisfy any impartial mind that some progress has been made towards the attainment of those objects for which the society was originally formed, and that by a proper application of our resources more may be done. The very crowded state of the Museum, which has hitherto prevented any systematic arrangement, and the want of a suitable Lecture Room, induced your Council to call the attention of the Members to the expediency of endeavouring to obtain a suitable House of our own. At a Special General Meeting called for this purpose, it was resolved that application should be made to the different Members of the Institution, to ascertain to what extent they would be willing to contribute to such an object. The result of these applications, as far as they have been made, has been such as to induce a strong hope that this great object may be accomplished.

Dr. Potts favoured the Society during the last winter with lectures on Phrenology and on Chemistry, but was, from the state of his health, prevented from continuing them to the extent he had proposed.

Mr. Hogg likewise gave two lectures on the Fabulous History of Cornwall, which were evidently the results of considerable research.

At a Special General Meeting in June last, it was resolved to enlarge the sphere of the Institution, by admitting Gentlemen residing at a distance, or Officers of his majesty's service, who may have favoured the Society with valuable literary or scientific communications or donations to the Museum, or from whom such assistance may be expected, as corresponding Members.—They are admitted to the Rooms, and to all lectures given by the Society.

In the hope and belief that Science will advocate its own cause, your Council look forward to a more favourable report of the state of your funds another year, than they are enabled

to

to make now. The expenditure has exceeded the receipts about 30*l.*, but a considerable proportion of this has arisen from contingencies, which are not likely to occur again, and a further portion from the expense of new cases, &c., to hold our increasing collection.

ROYAL ACADEMY OF SCIENCES OF PARIS.

Aug. 2.—The director of the administration of manufactures requested the Academy to examine a manuscript work by M. Charles Barbier, entitled An essay on the Chinese and Persian notography.—It was announced that the fossil bones of a very large mastodon had been discovered at Montpellier; and that M. Gambart, director of the observatory of Marseilles, had discovered, on the 27th of July, a new comet in the constellation Hercules.—M. Poisson made a report on the new tables of the moon, by M. Damoiseau.—M. Desmoulins read a memoir on the lachrymal organ and nervous system of the *Trigonocephali.*—M. Pouillet commenced the reading of a memoir on the high temperatures and on the heat which prevail at the surface of the sun.

Aug. 9.—M. le Baron Blin communicated, in manuscript, his Treatise on Harmony.—M. Cauchy, in the name of a commission, made a report on an analytical memoir by M. Libri, relating to the theory of numbers: [this memoir will be printed in the *Recueil des Savans Etrangers.*]—M. Vauquelin, in the name of a commission, gave a report on M. Lassaigne's memoir respecting the means of discovering hydrocyanic acid in the organs of animals poisoned by that re-agent.—M. Arago announced that M. Pons had discovered, on the 24th of July, the comet which M. Gambart noticed three days afterwards. —M. Bory-Saint-Vincent read a note on a new apparatus for drying plants for herbariums.—M. Bailly read a memoir on the cold intermittent fevers, and on the alteration of animal heat in those maladies.

Aug. 26.—M. Audibert communicated a memoir in which he explained the means of raising stagnant water above its level, by certain combinations of forces of water and air.—Dr. Lauth read a memoir on the lymphatic vessels of birds. M. Moreau de Jonnes read some geographical researches on manioc, and on the limits of its culture among the aborigines of the New World.—Some further observations by M. Gaillon were read, supplementary to his memoir on the animalcula which constitute the nutriment of oysters.

Aug. 23.—M. Bressy, a physician at Arpajon, communicated a manuscript entitled *Manosphore.*—M. de Beaujeu communicated an account of a new method of transporting soils

 in

in agriculture.—M. Chevreul read a memoir relative to the simultaneous action of oxygen gas and of alkalies on a great number of organic substances.

Aug. 30.—Mr. Walsh, of Cork, communicated a note on the line of the most rapid descent.—M. Fresnel made a report, in the name of a commission, on the new microscopes of M. Selligue.—M. Payen read a memoir on the pyrites found on the 19th of August 1824, in a gravel-pit at Grenelle, and on the power which several mineral substances possess of depriving vegetable bodies of their colour.—M. Runge read a memoir on the chemical characters of the plants which constitute the families of the *Dipsaceæ* and *Rubiaceæ*.

Sept. 6.—M. de Freycinet communicated an extract of a letter, dated Amboyna, Oct. 14, 1823, which he had received from M. Duperrey, commander of the discovery ship La Coquille. It contained some interesting details of the researches in geography and natural philosophy made in this expedition, as well as of the collections which have been formed.—M. Moreau de Jonnes communicated some particulars of the yellow fever which appeared in the Island of Ascension in 1823.—M. Bouvard stated the parabolic elements of the comet discovered at La Marlia and at Marseilles in July 1824; calculated from observations made at the Royal Observatory from August 3 to Sept. 3.—M. Desfontaines gave a verbal report on a work, entitled *Hortus Ripulensis, seu Enumeratio Plantarum quæ Ripulis coluntur ab* Aloysio Colla.—M. Thenard presented, in a verbal report, the results of the analysis which he had made, in conjunction with M. Vauquelin, of several fragments of the fossil found at Moret; and M. Cuvier, in relation to the same subject, made some remarks on the characters which belong to the organic remains of animals.—M. Navier made a verbal report on Mr. Stevenson's History of the Northern Lighthouses.—M. Geoffroy de Saint-Hilaire read a memoir, entitled On the composition of the skull in vertebrated animals, principally of crocodiles and birds: (art. 1.), of the *Cranium*, as forming part of the *rachis*, and as consisting of *seven vertebræ*.

[On Sept. 13 no meeting was held, on account of the illness of His Majesty Louis XVIII.]

Sept. 20.—M. Martillat announced some new improvements in his steam-engine.—A letter from Mr. Walsh was submitted to the examination of M. Cauchy.—M. Gaudin, of Nantes, communicated a memoir on equations of the 2d order.—M. Latreille made a verbal report on the *Analecta Entomologica* of M. Dalmane.—M. Fourier read a memoir, entitled General remarks on the temperatures of the terrestrial globe and

and the planetary spaces.—M. Bonard read a geological notice on certain districts of Burgundy.

Sept. 29.—M. Desfontaines made a verbal report on Sir James Edward Smith's English Flora.—M. Fourier concluded his memoir.—M. Geoffroy de Saint-Hilaire read a memoir, entitled On the sections of the cranium in the crocodile, compared with the analogous bones in all animals; restored, on the one hand, to philosophical identity, and considered, on the other, in the relation of the specific nature and the anomalies of their forms.

Oct. 4.—M. P. Coste, an officer in the artillery service, transmitted a sealed packet, inscribed "Chemical Researches." —M. de Schutten, professor of mathematics in the university of Abo, presented a memoir on the concussion of solid bodies. —M. le Baron Blin, communicated to the Academy a notice of his experiments with the *Syrene* of M. le Baron Cagniard de la Tour.—M. Geoffroy de Saint-Hilaire read the continuation of his memoir on the sections of the cranium.—M. Lamouroux read a memoir on the geography of marine plants.

Oct. 11. — M. Jomard communicated some information which he had received from M. Beaufort's expedition into the interior of Africa: he presented specimens of the butter of Schaa, and of the oil of palm-trees collected by that traveller. Dr. Lassis read a manuscript, entitled Notice on the causes of epidemic disorders, on the means by which it is said they may be resisted, and on some other points of medical science equally important.—M. Vauquelin gave a report on M. Laugier's analysis of three minerals brought from Ceylon and the coast of Coromandel by M. Leschenault de la Tour. — M. Marcel de Serres read a memoir on the fresh-water strata lately discovered in the vicinity of Cette, near the Mediterranean, and below the level of that sea.—M. de Bonard read a geological memoir on the plains of the Auxois.

Oct. 18.—M. Laugier read a memoir, entitled Chemical examination of the *Fer oxidé* (*résinite* of Haüy) found near Freiberg.—M. Becquerel read a memoir on the electro-dynamic effects produced during the decomposition of oxygenated water by various bodies, and on other phænomena caused by electricity in motion.—M. Brunel, of Varenne, communicated a new method of drawing.—M. Benoiston, of Châteauneuf, communicated a notice on M. Casper's work relating to the influence of vaccination on the population of the Prussian states.—M. Gaymard read some observations on certain mollusca and zoophytes, considered as the cause of the phosphorescence of the sea.

Oct. 26.—M. Gazil stated that he wished to submit to the judgement of the Academy a process of his invention for rendering

dering sea-water fresh.—M. Guion Desmoulins announced that he had discovered a liquid by which sculptures in marble might be cleaned without injury.—M. Durand, of Cherbourg, transmitted a memoir, entitled Notice on the formation of muriatic acid by means of nitric acid and carbon.—M. Moreau de Jonnes read a statistical note on the propagation and effects of a disorder which had been observed in various countries, and to which the name of *Varioloïde* had been given.—M. Maurice gave a verbal account of a work by M. Guillaume Libri on various points of analysis.—M. Duméril delivered a verbal report on M. Bremser's "Zoological and Physiological Treatise on the intestinal Worms of the Human Body."—M. Geoffroy de Saint-Hilaire presented a lithographic plate, entitled Determination of the sections of the cranium in fishes; composition of the skull in man and in animals.—M. Dupetit-Thouars read a notice on certain particularities of cotyledons and roots.—M. Serullas read a memoir on the amalgam of potassium, and on the electricity developed by its contact with water.

Nov. 2.—M. Becquerel announced that he had determined by experiment the intensity of the electro-dynamic force in any one point of a wire uniting the two extremities of a pile, and that it resulted from his researches that this intensity is constant for the entire length of the wire.—M. Benoiston de Châteauneuf, in his reply to a letter which had been addressed to him, announced that he still continued his researches on the influence of vaccination at Paris and in France.—MM. Chaptal and Thenard delivered a report on M. Payen's memoir on the pyrites of Grenelle, &c.—M. Magendie read a memoir, entitled Continuation of the series of experiments on the fifth pair of nerves.—M. Loiseleur des Longchamps read a memoir on the means of obtaining several crops of silk in a year, succeeded by some observations relative to the history of silk-worms.—M. Raspail read a memoir on the formation of the embryo in the *Gramineæ*.

Nov. 8.—MM. Payen and Chevalier read a note on the quantity of free phosphoric acid appreciable by means of turnsol.—M. Cordier made a verbal report on the *Traité élémentaire de Minéralogie*, by M. Beudant.—M. Dupin read a memoir, showing the advantages of machinery to the class of workmen.

Nov. 15.—MM. Dumeril, Cuvier, and Magendie, gave a report on M. Lauth's memoir relative to the lymphatic vessels of birds.—M. Silvestre gave a verbal account of a work by M. Marivault, On the Agricultural State of France, and the means of improving it.

Nov. 22.—M. Huzard junior, presented a work, the joint produc-

production of him and M. Pelletier, entitled Researches on the genus *Hirundo.*—M. J. J. Meunier announced, in a note, that he possesses the ancient art of painting and burning glass with all colours.—M. Geoffroy Saint-Hilaire presented a work, entitled Synoptic tables explaining the composition of the skull in man and in animals. — M. Latreille communicated an analytical table of the natural families of the animals constituting M. Cuvier's division of the Mollusca.—M. Vauquelin read a memoir, entitled A chemical examination of a green substance which is formed on the mineral waters of Vichy.—M. Arago communicated some experiments relative to the oscillations of the magnetic needle.—The Academy named M. Pelletier as a candidate for the chair of Materia Medica vacant at the School of Pharmacy of Paris.—Dr. Lasserre presented a memoir on the operation for the stone.—A memoir by M. Bonastre on the analysis of the balsam of Canada was submitted to MM. Vauquelin and Dulong.

Nov. 29.—M. Gaudin communicated a new method of the application of algebra to geometry.—M. Laurencet read a memoir on the structure of the brain.—M. Louyer Villermet read a Memoir on the number of deaths in France in the middling and poor classes of people.

XII. *Intelligence and Miscellaneous Articles.*

ERROR IN M. SCHUMACHER'S TABLES.

WE are authorized to state that in Professor Schumacher's *Astronomische Hülfstafeln* for 1825, the declination of δ *Ursæ Minoris* is set down one *minute* too much, throughout the whole year.

BORON, ITS PREPARATION, &c.

The readiest method of obtaining boron, without losing too much potassium, is to heat the potassium with fluo-borate of potash. Boron and silicium resemble each other in their properties nearly as sulphur and silicium, or as phosphorus and arsenic. I have produced sulphuret of boron; a white and pulverulent substance, which dissolves in water, yielding sulphuretted hydrogen gas. Boron burns in chlorine. The chloride of boron is a permanent gas which is decomposed in moist air, producing a dense vapour; and in water giving muriatic and boracic acids. It condenses one and a half times its volume of ammoniacal gas. *Berzelius—Bib. Univ.* xxvi. 277.

UNICORN.

UNICORN.

Among the curiosities so liberally sent by Mr. Hodgson, assistant to the resident at Katmandoo, to the Asiatic Society of Calcutta, is a large spiral horn said to belong to the unicorn, and with it drawings of the animal made by a Bhotea peasant. The drawings are stated to convey the true image of a living animal of the deer kind, out of the centre of whose forehead grows a horn of the description transmitted. The animal is described as gregarious, graminivorous, and its flesh good to eat. Its name is *chiro*; its colour bright bay, and its dwelling-place the plains of B'hote, beyond the Himalayah, and especially the woody tract of country situated a few days north-west of Digurche, known to the natives by the name of Chaugdung. The testimony of the poor Bhoteas, whom trade and religion bring down annually to Nepaul, appears to be uniform respecting the existence of this animal, but they hesitate about procuring it, though urged by the promise of a liberal reward. They declare that the *chiro* is too large and fierce to be taken alive, or to fall under their simple weapons; but they sometimes find the horns, naturally shed by the living, or remaining after the decay of the dead animal. These horns are dedicated to their divinities, and the one obtained by Mr. Hodgson was brought to Katmandoo to be suspended in the interior of the temple of Sumb'hoo Nat'h.—*Asiatic Journal.*

EARTHQUAKE IN PERSIA.

Letters from Shiraz announce, that on the 27th Chawal, 1239, which answers to the month of April 1824, there had been an earthquake, which lasted six days and six nights without interruption, and which had swallowed up more than the half of that unfortunate city, and overthrown the other, as was the case with the earthquake at Aleppo. Nearly all the inhabitants fell victims to this catastrophe; scarcely five hundred persons could save themselves. Other letters from Aborkoh announce that the same shock, but less violent, had been felt there. Kazroon, a city between Aborkoh and Shiraz, was swallowed up, with almost the whole of its inhabitants, in consequence of the same earthquake. All the mountains surrounding Kazroon were levelled by it, and no trace of them now remains.—*Asiatic Journal.*

THE CROCODILE OF THE GANGES.

Dr. C. Abel, of Calcutta, has investigated the structure and character of the *cummeer*, or Ganges crocodile, and compared it with its described congeners, from an individual of great size, measuring eighteen feet from the extremity of the nose

to

to the end of the tail. It had been destroyed by a spear driven into the neck at the junction of the head with the cervical vertebræ. In most of its external characters it agreed with the *crocodilus biporcatus;* except that the toes of the latter are represented by Cuvier and Lacepede as more or less united by membranes or webs; the hind feet of the crocodile proper, according to Cuvier, are palmated to the extremity of the toes. This character is wanting in the *cummeer*, in which the inner toe of the hind and two inner toes of the fore feet are perfectly free, not being connected by any membrane. If this peculiarity be of constant occurrence, it makes the *cummeer* not only a new and undescribed species, but it also vitiates the description of the family and of the genus of crocodile heretofore given.

Although the putrescency of the body of the animal prevented any deliberate examination of its internal structure, the contents of its stomach were exposed, and found to consist of the remains of a woman, of a whole cat, of the remains of a dog and sheep, of several rings, and of the separated parts of the common bangles worn by the native women.—*Asiatic Journal.*

DISCOVERY OF SELENIUM IN THE SULPHURIC ACID MADE FROM THE PYRITES OF ANGLESEA.

In our last volume we inserted a paper by Professor Scholz, of Vienna, on the extraction of selenium from the residuum of the sulphuric acid works at Lukawitz in Bohemia, where the sulphur employed is obtained from pyrites found in the vicinity of the place, a till then unknown locality of selenium.

We are glad to find, from a communication in the Annals of Philosophy for January, that seleniferous pyrites are also to be found in our own country. Mr. E. P. Thomson, a chemical manufacturer of Manchester, in making muriatic acid uses sulphuric acid prepared from the pyrites of the Paris Mountain in Anglesea by Mr. R. Mutrie, also of Manchester. The selenium, Mr. Thomson has observed, distils over with the muriatic acid into the receivers, and in the course of two or three days it falls to the bottom of the vessels in the form of a reddish-brown substance, which does not appear to deteriorate the acid in the least. The quantity yielded by the acid is very small. Mr. Children having submitted a portion of this red substance to experiment, in order to obtain unequivocal evidence of its containing selenium, gives the following statement of his results:

A fragment heated on a slip of platina foil by the spirit lamp, tinged the flame of a beautiful azure blue colour. A

portion heated by the spirit lamp in a glass tube closed at one end gave off first acidulous water; some sulphur next sublimed and condensed at a little distance from the flame, and soon after a red substance, which condensed on the sides of the tube between the flame and the sulphur, and very near the former. During the sublimation of the red matter, the lower part of the tube was filled with a yellow vapour, a good deal like chlorine, but of a deeper colour, and an unpleasant odour was exhaled, very similar to that of cabbage water. After the whole of the volatile matter had been sublimed, a fixed dark-coloured residuum remained at the bottom of the tube. This was transferred to another tube, open at both ends, and again heated; some more of the red sublimate was thus obtained, and the residuum assumed a grey colour. It amounted to about 53 per cent. of the weight of the substance operated on, and on examination was found to consist of earthy matter, principally silica and lime; consequently the assay contains about 47 per cent. of volatile matters, by far the greatest portion of which consists of the red sublimate. The red sublimate had evidently been fused and spread over the inner surface of the tube.

When detached from the tube, a morsel of it imparted the same beautiful blue colour to flame that has been already mentioned, but more intense.

Another fragment, heated in a tube open at both ends, sublimed without giving off any sulphur, exhaling at the same time a strong odour similar to that of horse-radish. It fused very readily on being gently heated in a close tube over the lamp, and remained for some time in a soft pasty state.

These experiments are quite sufficient to establish the identity of our red sublimate with selenium, and in external characters also it perfectly answers the description of that substance. It has a metallic lustre, and a deep brown colour when seen by reflected light. Its fracture is conchoidal, and has a vitreous lustre. It is easily scratched by the knife, is brittle, and its powder has a deep red colour; but it adheres together readily when rubbed in the mortar, and then assumes a grey colour, and a smooth and somewhat metallic surface. In very thin laminæ it is transparent, and when viewed by transmitted light has a beautiful cinnabar red colour.

ACCOUNT OF AN EXPLOSION IN A VEIN OF PYRITES.

This explosion took place, sixteen years ago, in the township of Yonge, near the Lake of the Thousand Isles in the St. Lawrence. At the time, a man was seeking his cow in the woods, within a short distance of the spot. On a sudden he

was

was startled by a tremendous explosion, attended by volumes of smoke and sulphurous odours.

Three years since, on being informed of these particulars, Dr. Bigsby visited the place. It is half a mile within the woods north of the road from Brookville to Kingston, near to the easternmost of two creeks, about ten miles from the former town.

He found, on the summit of a quartzose mound from 30 to 40 feet high, a round cavity, 12 feet deep, 12 long, and 9 broad. Its sides consisted of very shattered quartz, spotted brown by oxide of iron, and covered profusely with acicular yellow and white crystals of sulphur. The lower parts of the cavity were studded with masses of iron pyrites, of which there is a vein at the bottom of the cavity. It is a foot and a half thick, and disseminates itself into the surrounding quartz. This vein may be seen, running east with a very high dip, to the distance of a yard and a half.

Similar phænomena have been noticed in a mountain in Vermont (*vide* American Journal of Science for Feb. 1821), and in the country towards the head of the Missouri (*vide* Travels of Captains Lewis and Clarke).—*Geol. Trans.* p. 209.

ON RED SANDSTONE.

As much discussion has taken place among geologists within these few years respecting the various formations of *Red Sandstone*, we think it may be useful to give the opinion on the subject of Professor Buckland and the Rev. W. D. Conybeare, as stated, with a concise but comprehensive view of its history, in their Observations on the South-western Coal District of England; Geol. Trans. sec. ser. p. 314.

In the south-western coal-district of England we have three formations of red sandstone, the *newer red sandstone*, the *millstone grit*, and the *old red sandstone*, all liable to be confounded with one another, owing to their prevailing red colour and to their containing beds of conglomerate; and as similar rocks occur, very similarly placed, in various parts of the earth's surface, we find three opinions maintained concerning red sandstone, and each moreover supported by indisputable facts: one, that it lies over the coal-measures; another, that it lies beneath them; and a third, that it is a member of the coal-formation.

The term *old red sandstone* was originally applied by Werner to a formation analogous in character and geological position to our *newer red sandstone*. Examples of the *rothe todte Liegende*, or old red sandstone of Werner, lying over the coal-measures, may be seen at Norhausen on the borders

of the Hartz, and at the Wintberg mountain, a few miles to the south-west of Dresden, on the edge of the Dresden coal-field. It is to this overlying formation of red sandstone that, in our opinion, the associated presence of large masses of salt and gypsum is exclusively confined.

The *old red sandstone* of English geologists, and the mountain limestone which covers it, great as is the thickness and importance of each formation, are not recognised in the classification of rocks which Werner himself has drawn up. An example of both these rocks, identical with their types in England, and emerging from beneath the coal-measures, may be seen at Huy in the district of the Meuse, between Namur and Liege.

The *millstone grit* affords the best example in the south-western coal-field of a red sandstone belonging to the coal-measures. But occasionally even in this coal-field, and very frequently in the coal-districts on the continent, all the coal-grits acquire a red colour; and for this reason we now find it to be the prevailing opinion among continental geologists, that the *grès rouge* is a member of the coal-formation.

It is by their relative position to one another as well as to other rocks, that these sandstones are best to be distinguished; but when these points remain obscure, we must have recourse, for the purpose of discrimination, to some of the internal characters detailed in the preceding memoir; such, for instance, as can be observed in the conglomerate beds from the nature of their imbedded fragments, which, should they be indubitable fragments of old red sandstone, mountain limestone, and the coal-measures, would lead us to refer the disputed formation to the newer red sandstone.

The distinguishing characters and relative position of these three formations of red sandstone having been only partially attended to, and much confusion having thence arisen,—in order to remove it, an eminent geologist has proposed the expedient of throwing them all together, and regarding them as belonging to one formation of sandstone, in which are contained subordinate beds of limestone and coal. This view of the subject may appear at first sight to introduce an advantageous simplification; but for the following reasons we cannot consent to adopt it.

With regard to the grits of the coal-measures and of the old red sandstone, since they lie conformably to one another, it may sometimes perhaps be found convenient, in an extended sense, to class them under one formation; and should it happen that both are of a red colour, and (as is the case in Shropshire) that the mountain limestone, which usually divides them,

has

has disappeared, it may then be difficult in fact to distinguish between them. But between both of these and the newer red sandstone there is a complete and total separation. To those rocks the newer red sandstone is unconformable in position, and whenever in contact with them reposes on their basset edges: it is usually unaffected by the faults and disturbances to which they are subject; it is itself partly made up of the fragments derived from their ruins; and must therefore have been deposited after their consolidation, dislocation, and partial destruction. It is impossible, therefore, to imagine any stronger grounds on which any two series of rocks can be regarded as distinct: and should we agree to throw these together, we might with equal propriety consider all groups of strata, in which beds of limestone occur, as belonging to one great calcareous formation, and treat as subordinate all the rocks that happen to alternate with limestone. But this would be in fact to confound together almost all the rocks with which we are acquainted.

PRODUCE OF THE COPPER MINES OF GREAT BRITAIN.

Quantity of copper raised from the mines of Great Britain in the last six months, ending December 31, 1824:

	Quantity of Ore. Tons.	Quantity of Copper. Tons. cwt. qrs.
Mines in Cornwall . . .	53,514	. . . 4,119 16 2
———Devon	3,030	. . . 308 1 2
Various mines, including Ireland, sold in ore at Swansea	2,598	. . . 250 12 3
	59,142	4,678 10 3
Anglesea and Staffordshire estimated at .	. . . .	350 0 0
		5,028 10 3

The 4427 tons, 18 cwt. of fine copper raised in Cornwall and Devon is the produce of 80 mines, of which the following six are the principal:

	Ores.	Fine Copper.
Consolidated Mines . . .	7767 tons	712 tons.
East Crinnis	3677 . .	309
Wheal Buller and Wheal Beauchamp	3328 . .	227
Wheal Friendship (Devon)	1757 . .	220
Pembroke	4221 . .	216
Dolcoath	3418 . .	215
		——1899 tons.

Copper ores are weighed at 21 cwt. to the ton, and fine copper at 20 cwt.

OPENING

OPENING OF A MUMMY.

On Thursday evening, 9th Dec., was unwrapped at the Bristol Institution the body of an Egyptian mummy, which it is understood was removed by Mr. Salt from a catacomb in the Thebais, and sent down the Nile to Alexandria, and thence to Bristol. The case, which was beautifully covered with hieroglyphics, exhibited rather the copper-coloured countenance of a Nubian, than the expanded forehead and wide eye-sockets of an Ethiopian. The upper part of the shell being removed, there arose a peculiar, but not unpleasant, odour. The body was remarkably light, and wrapped up in a multitude of folds of cotton cloth, which was stained of a yellowish brown colour. Upon the removal of the circular bandages, there appeared a long wrapper from the chin to the toes, with a double border of blue stripes in front. The innermost layer of cloth was soaked in naphtha, asphaltum, or some bituminous substance, combined probably with natron. The skin was blackened, and the neck and one of the hands had been attacked by a peculiar sort of coleopterous insect, apparently a dermestes. In other respects, this curious specimen of antiquity was very perfect, indeed much more so than usually happens. It was the body of (probably) a young female. The hands were placed straight upon the thighs, and not, as most frequently happens, across the bosom. The hair upon the head was perfect, of a brownish auburn colour, short, but not at all wearing the character of a negro's. The contour of the countenance strengthened the opinion that the subject belonged to a province closely bordering upon the confines of Egypt. The coverings of the chest and stomach being removed, exhibited, in high preservation, the heart and lungs, and all the intestines: indeed it did not appear that any part had been removed. Whether the brain had been extracted was not ascertained; neither were the teeth examined, as it was thought advisable to subject the head altogether to a more leisurely and minute observation.

EARTHQUAKE IN SUSSEX.

On the 6th of Dec., a few minutes before two o'clock in the afternoon, a shock of an earthquake was very generally felt at Portsmouth and its neighbourhood, also at Havant, Emsworth, and Chichester. The shock, although it was not accompanied by any report, put both light and heavy furniture in a tremor for about four seconds of time. The floors seemed to heave up a little, the windows in consequence shook as they do by means of heavy gusts of wind; and suspended articles, as bird-

cages,

cages, &c., oscillated some seconds after the shock had subsided. There was no unusual appearance in the state of the sky, or about the sun at the time; but during the morning the sky had been filling with light clouds, and soon after the shock a stratum of low electric clouds sprang up with the wind from the S. W., and the upper stratum changed from a grey to red and lake colours some time before the sun had set. If the shock was not caused by the explosion of a meteor, it probably was the effect of the earth sinking somewhere in the southern part of England, in consequence of the great quantity of rain that has fallen during the last three months. It is now about twelve years since the last shock was felt here, which occurred in the night, and was more violent than this one.

Chichester, Dec. 8.

The earthquake here on Monday was very alarming. Many families ran out of their houses in great trepidation; several people felt the chairs move under them. The bells rang in many houses, and glasses jingled. In the market-house, apples were seen rolling off the stalls. At Aldwick and Bognor it was also very sensibly felt, as likewise in the parts adjacent.

Another account.—Monday afternoon, about two o'clock, the shock of an earthquake was very sensibly felt in the city of Chichester, which alarmed a considerable portion of its inhabitants, many of whom ran into the streets in the greatest consternation, under the impression that their dwellings were actually falling. In several houses the bells were set a-ringing, and the window-blinds unrolled; and one individual states, that he was sitting in a small room, and distinctly saw the walls move from south to north out of their perpendicular, and as instantaneously resume their position. The shock lasted from three to five seconds. We have not heard that it was felt further in this direction than the vicinity of Arundel. Many persons imagine that there has been an earthquake in some place abroad, as in the year 1812 a shock, but much slighter, was experienced in Chichester; which at the time proved so destructive to property in the city of Caraccas, South America.—*Hampshire Teleg.*

ROPE BRIDGES IN INDIA.

These bridges are called Portable Rustic Rope Bridges of Tension and Suspension, and they are exactly what the name describes. A few hackeries will carry the whole materials, and the appearance of the bridge is rustic and picturesque. They are distinctly bridges of tension and suspension, having no support whatever between the extreme points of suspension independent of the standard piles, which are placed about fifteen feet from the banks of the nullah, or river, except what they

they derive from the tension, which is obtained by means of purchases applied to a most ingenious combination of tarred coir ropes of various sizes, lessening as they approach the centre. These form the foundation for the pathway, and are overlaid with a light split bamboo frame-work. The whole of this part of the fabric is a fine specimen of ingenuity and mathematical application. One great advantage it possesses is, that if by any accident one of the ropes should break, it might be replaced in a quarter of an hour, without any injury to the bridge. It is impossible in this article to give so particular a description as to render its minute parts clear, nor in fact can any description do so unaccompanied by the plan.

The chief principle of its construction is the perpendicular action of its weight, a principle obviously of paramount necessity in this country, where the soil is so loose, and offers so little resistance—and more particularly in relation to the specific purpose for which they were invented. The whole weight of the bridge, therefore, resting on two single points, so far separated, and unassisted either by pier-head or abutment, rendered its construction a matter of extreme delicacy, and it has been effected in a manner reflecting the highest credit on the genius of the inventor. The combination of lightness with security, and the adaptation, to the utmost nicety, of the required proportionate strength to the parts, form its chief characteristics. The tension power is wholly independent of the suspension.

The bridge which was placed during the last rains over the Berai torrent was 160 feet between the points of suspension, with a road-way of nine feet, and was opened for unrestricted use, excepting heavy-loaded carts. The mails and banghees passed regularly over it, and were by its means forwarded when they would otherwise have been detained for several days. The last rainy season was the most severe within the last fifty years, and yet the bridge not only continued serviceable throughout, but on taking it to pieces it was found in a perfect state of repair. The bridge intended for the Caramnassa is 320 feet span between the points of suspension, with a clear width of eight feet. It is in other respects the same as the Berai torrent bridge. A six-pounder passes over with ease; six horsemen also passed over together, and at a round pace, with perfect safety.

We have no doubt but that these bridges will eventually become general. During the rains there will be three of them on the great military north-west road to Benares, and we feel satisfied their utility will be finally established at the conclusion of the season.—*Calcutta John Bull.*

RUSSIAN

RUSSIAN CHAIN-BRIDGE.

A chain-bridge, the first of its kind in Russia, is about to be constructed over the canal of Moika. It will be executed after the design of Colonel Dufour, of Geneva, who has sent to St. Petersburgh a correct model of one which he erected in his own country last year.

RAIL-ROADS—LOCOMOTIVE STEAM-ENGINES.

On the 17th instant a grand experiment as to the power of locomotive engines was performed at Killingworth Colliery, near Newcastle-upon-Tyne, in presence of several gentlemen from the committees of the intended Manchester and Liverpool and Birmingham and Liverpool rail-road companies—when the result was as follows: The engine being one of eight-horse power, and weighing, with the tender (containing water and coals), five tons and ten hundred weight, was placed on a portion of rail-road, the inclination of which, in one mile and a quarter, was stated by the proprietor Mr. Wood to be one inch in a chain, or one part in 792: twelve waggons were placed on the rail-road, each containing two tons and between 13 and 14 hundred weight of coals—making a total useful weight of 32 tons and 8 cwt. The twelve waggons were drawn one mile and a quarter each way, making two miles and a half in the whole, in forty minutes, or at the rate of 3¾ miles an hour, consuming four pecks and a half of coals. Eight waggons were then drawn the same distance in thirty-six minutes, consuming four pecks of coals; and six waggons were drawn over the same ground in thirty-two minutes, consuming five pecks of coals. Our correspondent also mentions, that the engine must be supplied with hot or boiling, and not with cold water; and that two hundred gallons of water will take the engine 14 miles, at the end of which the supply must be renewed. —*Morn. Chron.*

Calendar of Flora, Fauna, and Pomona, at Hartfield in Sussex (continued), from December 20, 1824, *to January* 20, 1825.

Dec. 20.—Weather mild and moist. The following plants in flower here and there: *Tussilago fragrans* in great abundance; Polyanthus, Primrose, Periwinkle, Leopard's-bane, Marigold, Laurustine, Wallflower, and Stock.

Dec. 21 and 22.—Wind and rain, and very damp. Very few berries on the Holly, and in general the hedges are very bare of berries. Misletoe in abundance: one was brought to me of immense size.——Dec. 25. Christmas Day.—The Christmas Rose or White Hellebore is coming into flower.

Dec. 28.—Much wet, and floods in the meadows.

Dec. 31.—*Helleborus hyemalis* in full flower. Weather wet,

wet, windy and mild. A flower or two on old Marigolds and on the Purple Hepatica. Daisies in abundance.

1825. Jan. 3.—It is very unusual at this time to have such a number of plants bearing flowers as we now have: The Great Leopard's-bane, Polyanthus, Primrose, *Lychnis dioica*, *Antirrhinum Cymbalaria*, *Galium Mollugo*, *G. palustris*, and *G. Aparine*; *Thlaspi Bursa Pastoris*, and many Confervas.*

Jan. 8. — Barometer as high as 30·60†; a clear day again. This winter in one respect resembles that of 1816-17; namely, that we have rapid changes from slight frost to warm rain without any permanent weather.——Jan. 20.—Thrush, Redbreast, and other birds begin to sing‡.

* On Friday, January 7th, the bees were flying about in the garden of Rose Mount. Sunday the 9th resembled a day in March. The sky was without a cloud, there was scarcely a breath of wind, and in the country in the morning the Blackbirds were singing as if welcoming the spring. The pastures have a fine, fresh and healthy appearance, the wheat braird is strong, thick in the ground, and nearly covers the soil. Vegetation is going on in the gardens, and the usual spring flowers are making their appearance. The Christmas Rose, the Snowdrop, the Polyanthus, the single or border Anemone, the Hepatica in its varieties, and the Mezereon are in full bloom. The Narcissus is making its appearance, and the Crocuses are showing colour. This morning, at six o'clock, the thermometer in Nelson-street indicated 44 degrees. On Sunday the barometer gained the extraordinary height of 31·01; this morning it is at 30·8.—*Glasgow Chronicle.*

† On the 9th the barometer with us reached 30·80, and is said to have been as high at Worcester as 30·96.—Edit.

‡ The Editor of the *Norwich Mercury* says, There is now standing upon the mantle-piece of the room in which we write, a glass containing a Rose, a Pink, Primroses, Violets, Polyanthuses, Stocks, and Wall-flowers, all grown in the open air, and plucked from our garden a day or two ago. On the 17th we heard the Thrush singing for the first time this spring-winter.—*Costessey*, Jan. 20.

The winter has been remarkable for mildness on the other side of the Atlantic, as appears from the following extract:—"We cannot call to mind in our own time a solitary instance of the same continued mildness of weather at the same season of the year. In almost every section of the country the weather seems to have been equally pleasant.—The *Savannah Republican* says, 'The beautiful idea of the poet—of Winter lingering in the lap of May'—is at this time completely transposed in our climate, for May is smiling in the arms of December. Our thermometers are more than thirty degrees above the usual freezing point of the season. The grass begins to dress itself in green; the sweet jessamine and woodbine in the gardens of our city have expanded their fragrant leaves, and present to our view full-bloom flowers; the rose partially covers its stem with luxuriant leaves, and the infant bud of Flora's favourite modestly begins to peep forth through the sheltering foliage; the trees of every description start their buds to join the jubilee; the peach is in full bloom, and the mocking-bird, the early messenger of spring, chants forth her praises for the continuance of mild and congenial airs.'—Both Savannah and Darien papers speak of ripe mulberries and damsons. Peaches have already swelled to the size of a nutmeg.—The North River is nearly if not quite free from ice as far as Troy."—*New York Commercial Advertiser*, Dec. 31.

The

ZOOLOGY OF SCOTLAND.

The number of vermin killed by Richard Burniston, the gamekeeper to Lord Gwydyr, on the district of Callander, Perthshire, from December 1823 to December 1824, gives us a good idea of this branch of natural history on the Highland Border:—4 foxes, 1 otter, 9 badgers, 29 martin cats, 11 wild cats, 22 pole cats, 1 stoat, 2 weasels, 12 hedgehogs, 61 house cats, 111 gledes, 105 ravens, 22 hawks, 136 hooded crows, 2 owls, 3 daws, 31 magpies, 11 jays. The above quantity, 573 head, were killed with traps precisely in one year.

LIST OF NEW PATENTS.

To William Francis Snowden, of Oxford-street, in the parish of St. George, Hanover-square, Middlesex, machinist, for his invented wheel-way and its carriage or carriages for the conveyance of passengers, merchandize, and other things along roads, rail and other ways, either on a level or inclined plane, and applicable to other purposes.—Dated 18th of December 1824. —6 months to enrol specification.

To John Weiss, of the Strand, Middlesex, surgical-instrument maker and cutler, for certain improvements on exhausting, injecting, or condensing pumps or springs, and on the apparatus connected therewith, and which said improvements are applicable to various useful purposes.—18th December.—6 months.

To James Deykin and William Henry Deykin, of Birmingham, button-makers, for an improvement in the manufacture of military and livery buttons.—23d December.—2 months.

To Daniel Stafford, of Liverpool, for improvements on carriages.—24th December.—6 months.

To Samuel Denison, of Leeds, whitesmith, and John Harris, of Leeds, paper-mould maker, for improvements in machinery for the purpose of making wove and laid paper.—1st January, 1825.—6 months.

To Pierre Erard, of Great Marlborough-street, Middlesex, musical-instrument maker, for certain improvements in piano-fortes.—5th January.—6 months.

To Alexander Tilloch, LL.D., of Islington, for improvements in the steam-engine or apparatus connected therewith.—11th January.—6 months.

To William Henson and William Jackson, both of Worcester, lace-manufacturers, for improvements in machinery for making bobbin-net.—11th January.—6 months.

To Goldsworthy Gurney, of Argyle-street, Hanover-square, surgeon, for his improved finger-keyed musical instrument, in the use of which a performer is enabled to hold or prolong the notes and to increase or modify the tone.—11th January.—6 months.

To Francis Gybbon Spilsbury, of Leek, Staffordshire, silk-manufacturer, for improvements in weaving.—11th January.—6 months.

To William Hirst, of Leeds, cloth-manufacturer, for improvements in spinning and shabbing machines.—11th January.—6 months.

To John Frederick Smith, of Dunston Hall, in the parish of Chesterfield, Derbyshire, esquire, for improvements in the preparation of slivers or tops from wool, cotton, or other fibrous materials.—11th January.—6 months.

To John Frederick Smith, of Dunston Hall, Chesterfield, esquire, for improvements in dressing and finishing woollen cloths.—11th January.—6 months.

To James Falconer Atlee, of Marchwood, county of Southampton, for a process by which planks and other scantlings of wood will be prevented from shrinking, and will be altered and materially improved in their durability, closeness of grain, and power of resisting moisture, so as to render the same better adapted for ship-building and other building purposes, for furniture and other purposes where close or compact wood is desirable; insomuch that the wood so prepared will become a new article of commerce and manufacture, which he intends calling "condensed wood."—11th Jan.—6 months.

To George Sayner, of Hunslet, in the parish of Leeds, Yorkshire, dyer, and John Greenwood, of Gomersall, in the said county, machine-maker, for improvements in the mode of sawing wood by machinery.—11th January.—6 months

To Thomas Magrath, of Dublin, for his composition to preserve animal and vegetable substances.—11th January.—6 months.

To Thomas Magrath, of Dublin, for his improved apparatus for conducting and containing water and other fluids, and preserving the same from the effects of frost.—11th January.—6 months.

To John Phipps, of Upper Thames-street, stationer, and Christopher Phipps, of River, Kent, paper-maker, for improvements in machinery for making paper.—11th January.—6 months.

To William Shelton Burnet, of London-street, London, for a new method of lessening the drift of ships at sea and protecting them in gales of wind.—11th January.—6 months.

To Jonathan Andrew, Gilbert Tarlton, and Joseph Shepley, of Crumpshall, near Manchester, cotton-spinners, for improvements in the machine used for throstle and water spinning of thread or yarn, which improved machine is so constructed as to perform the operations of sizing and twisting in or otherwise removing the superfluous fibres, and of preparing a roving for the same.—11th January.—6 months.

To John Heathcoat, of Tiverton, lace-manufacturer, for improvements in machinery for making bobbin-net.—12th January.—6 months.

To William Booth and Michael Bailey, of Congleton, Cheshire, machinists, for improvements in spinning, doubling, throwing, and twisting silk, wool, cotton, flax, &c.—13th January.—6 months.

To Joseph Lockett, of Manchester, engraver to calico-printers and copper-roller manufacturer, for improvements in producing a neb or slob in the shell or cylinder, made of copper or other metal, used in the printing of calico, &c.—14th January.—2 months.

To William Rudder, of Egbaston, near Birmingham, cock-founder, for certain improvements in cocks.—18th January.—6 months.

To William Church, of Birmingham, for improvements in casting cylinders, tubes, and other articles of iron and other metals.—18th January.—6 months.

METEOROLOGICAL REGISTER AT WICK.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

As you have announced to the public that the Meteorological Register kept at Wick, and published in the last Number of your valuable Magazine, came from me, it is but right that I should name its author—Kenneth MacLeay, Esq. of Newmore, who has for some years past been sedulously occupied in studying meteorology at the northern extremity of our island.

I am, &c. W. S. MacLeay.

SUMMARIES

Summaries of Meteorological Observations for the past Year;

[Continued from vol. 63. p. 76.]

Results of a Meteorological Register kept at New Malton, in the N. R. of Yorkshire, in the Year* 1824, *by* James Stockton, *Esq.

Latitude 54° 8′ 3″. Longitude 0° 47′ 4″ W.

Height of the Cistern of the Mountain Barometer above the Level of the Sea 92 feet.

The Funnel of the Rain-gauge is 7, and the Thermometer 3 feet from the Ground.

1824.	Barometer.						Thermometer.				Winds.											Weather.			Rain.	
Months.	Max.	Min.	Mean.	Range.	Spaces described in Inches.	No. of Changes.	Max.	Min.	Mean.	Range.	N.	NE.	E.	SE.	S.	SW.	W.	NW.	Var.	Brisk.	Boist.	Wet.	Snow.	Hail.	in Inches, &c.	Character of each Month.
Jan.	30·78	28·57	30·015	2·21	8·32	11	53°	26°	36°·025	27°	2	...	...	2	4	14	4	5	...	...	1	5	1	...	1·16	Calm, cloudy, and dry
Feb.	30·35	28·95	29·744	1·40	5·71	12	53	28	38·448	25	1	7	6	1	2	5	6	1	...	2	...	5	1	...	1·22	Ditto
March	30·25	28·74	29·704	1·51	9·72	16	56	22	38·516	34	9	7	1	...	6	4	2	...	2	10	4	7	6	...	2·83	Cold, wet, and changeable
April	30·46	29·29	29·832	1·17	5·92	12	64	29	45·133	35	3	7	...	4	5	2	1	5	3	2	4	6	3	2	2·11	Changeable
May	30·56	29·62	29·930	·94	3·66	9	72	30	51·080	42	9	7	...	3	2	2	2	...	6	3	2	10	...	1	2·27	Cold and wet
June	30·34	29·28	29·750	1·06	3·98	9	76	40	56·400	36	8	12	2	...	1	1	2	...	4	4	1	10	...	...	2·72	Fine to the 14th, then wet
July	30·38	29·41	29·931	·97	3·96	10	86	48	62·435	38	2	2	2	2	1	6	10	5	1	1	1	5	...	...	1·17	Warm and dry
August	30·55	29·36	29·880	1·19	4·16	11	72	46	59·500	26	7	7	...	1	...	4	8	1	3	1	1	11	...	...	2·85	Ditto
Sept.	30·19	29·22	29·807	·97	5·00	15	81	27	56·066	54	8	...	...	3	4	7	1	4	3	2	5	11	...	1	5·61	Wet, windy, and changeable
Oct.	30·05	28·85	29·550	1·20	6·89	18	66	30	47·404	36	4	2	1	2	4	7	4	6	1	5	5	15	1	...	6·25	Excessively wet, and stormy
Nov.	30.05	28·21	29·440	1·84	10·75	18	59	25	42·483	34	1	1	1	1	8	7	8	2	1	9	10	20	...	...	4·38	Excessively windy, and wet
Dec.	30·28	28·55	29·656	1·73	11·00	21	55	23	38·706	32	4	...	...	...	1	11	11	2	2	3	5	12	3	1	4·17	Wet and changeable.
Annual Means, &c.	30·78	28·21	29·770	2·57	79·07	162	86	22	47·683	64	58	52	13	19	38	70	59	31	26	42	39	117	15	5	36·74	

ANNUAL

ANNUAL RESULTS.

Barometer.

			Inches.
Highest observation, Jan. 16th.	Wind N.W.	...	30·780
Lowest observation, Nov. 23d.	Wind S.E.	...	28·210
Range of the mercury		...	2·570
Mean annual barometrical pressure		...	29·770
Greatest range of the mercury in January ...		...	2·210
Least range of the mercury in May		...	·940
Mean monthly range of the mercury		...	1·349
Spaces described by the different oscillations		...	79·070
Total number of changes in the year		...	162·000

Six's *Thermometer.*

Greatest observation, July 14th.	Wind S.E.	...	86°000′
Least observation, March 3d.	Wind N.	...	22 000
Range of the mercury in the thermometer		...	64 000
Mean annual temperature		...	47 683
Greatest range in September		...	54 000
Least range in February		...	25 000
Mean monthly range		...	34 916

Winds.

	Days.		Days.
North	58	West	59
North-East	52	North-West	31
East	13	Variable	26
South-East	19	Brisk	42
South	38	Boisterous	39
South-West	70		

Rain, &c.

	Inches, &c.
Greatest quantity in October	6·25
Least quantity, January and July	1·16
Total amount for the year	36·74
Days of rain	117·00
Days of snow	15·00
Days of hail	5·00

Remarks.—The mean temperature of the year just elapsed very nearly corresponds with that of 1819, and the amount of rain, which is about 6 inches less than in the preceding year, is on a similar par with that for 1822. Upwards of 20 inches it will be observed have fallen since the 1st of September, two-thirds of which fell by night, and frequently attended with most boisterous gales. J. S.

New Malton, January 3, 1825.

A METEORO-

METEOROLOGICAL TABLE.

Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat. 56° 23′ 30″.—Above the level of the Sea 129 feet.

	Morning, 10 o'clock. *Mean height of*		Evening, 10 o'clock. *Mean height of*		Mean Tempr. by Six's	Depth of Rain.	N° of Days.	
1824.	Barom.	Ther.	Barom.	Ther.	Ther.	Inch. 100	Rain or Snow.	Fair.
January . .	29·799	41·193	29·829	41·322	41·933	1·35	8	23
February. .	29 710	40·483	29·700	39·517	40·862	1·45	11	18
March. . . .	29·660	39·774	29·670	37·548	39·451	1·05	11	20
April	29·779	46·980	29·728	43·366	45·370	1·00	9	21
May	29·915	52·677	29·901	47·258	50·710	·40	6	25
June. . .	29·858	58·533	29·83[illegible]	52·700	56·400	1·75	9	21
July	29·802	60·387	29·800	56·419	59·420	1·80	9	22
August . . .	29·798	58 710	29·787	54·[illegible]58	57·450	1·70	16	15
September.	29·743	54·800	29·714	52·066	53·960	2·20	13	17
October. . .	29·517	47·451	29·502	45·322	47·255	4·00	22	9
November.	29·317	40·966	29·320	40·[illegible]00	41·423	4·40	18	12
December .	29·440	38·677	29·434	38·061	39·451	2·90	16	15
Average of the year.	29·695	48·386	29·685	45·670	47·808	24·00	148	218

ANNUAL RESULTS.

MORNING.

Barometer.				*Thermometer.*	
Observations.		*Wind.*		*Wind.*	
Highest, 16th Jan.		SW.	30·54	14th July, SW.	68°
Lowest, 8th March,		E.	28·41	4th December. W. . .	25°

EVENING.

Highest, 15th Jan.	W.	30·55	2d September, SE. . . .	65°
Lowest, 23d Nov.	E.	28·40	4th December, W.	22°

Weather.	*Days.*	*Wind.*	*Times.*
Fair	218	N. and NE.	15
Rain or Snow	148	E. and SE.	110
	366	S. and SW.	55
		W. and NW.	186
			366

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 5th December	Wind W. . . .	21°
Hottest, 14th July	Wind SW. . .	75°
Mean Temperature for 1824		47° 808′

RESULT OF TWO RAIN GAUGES.	In. 100
1. Centre of the Kinfauns Garden, about 20 feet above the level of the Sea	24·00
2. Kinfauns New Castle, round Tower, about 150 feet . . .	20·18

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport, Mr.* CARY *in London, and Mr.* VEALL *at Boston.*

Days of Month, 1824 and 1825.	Gosport, at half-past Eight o'Clock, A.M. Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Clouds. Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Height of Barometer, in Inches, &c. Lond. 1 P.M.	Bost. 8¾ A.M.	Thermometer. London. 8 A.M.	Noon.	11 P.M.	Boston. 8¾ A.M.	Rain. London.	Boston.	Weather. London.	Boston.	Wind.
Dec. 26	29·86	44	51·80	76	NW.	...	......	1	...	1	...	1	...	...	29·96	29·55	40	44	40	40·5	...	...	Fair	Stormy	W.
27	29·93	50	......	80	SW.	...	0·110	1	...	1	...	1	1	1	29·86	29·60	47	53	50	44	...	...	Fair	Stormy	W.
☽ 28	29·82	52	......	82	SW.	0·12	·400	...	...	...	...	...	...	1	29·80	29·50	47	52	42	48·5	...	...	Rain	Cloudy	SW.
29	30·00	41	......	78	N.	...	......	1	1	1	...	...	...	...	30·20	29·85	40	40	34	33	...	...	Fair	Fine	Calm
30	30·18	48	......	87	SW.	...	·015	1	...	1	...	...	...	1	30·19	29·85	45	52	47	47	...	...	Fair	Cloudy	Calm
31	30·22	49	......	80	SW.	·05	·080	1	1	...	...	...	1	1	30·17	29·80	47	50	47	47	...	...	Cloudy	Stormy	SW.
Jan. 1	30·11	51	51·55	88	W.	...	·150	1	...	1	...	...	...	1	30·19	29·60	50	55	52	51	...	...	Fair	Stormy, rain *p.m.*	W.
2	29·91	46	......	75	W.	...	......	1	...	1	...	1	...	1	29·97	29·55	46	46	38	40	...	·10	Fair	Fine	W.
3	30·34	41	......	74	NW.	·05	·040	1	1	1	...	...	...	1	30·30	30·07	40	44	50	47	0·65	...	Cloudy	Cloudy	Calm
○ 4	30·00	52	......	80	W.	...	·005	1	...	1	...	...	1	1	30·01	29·60	50	55	35	51·5	...	...	Cloudy	Cloudy	SW.
5	30·48	33	......	73	N.	...	......	...	...	1	...	...	1	...	30·60	30·40	34	38	33	33·5	...	...	Cloudy	Cloudy	Calm
6	30·59	32	......	75	E.	·15	......	1	...	1	1	1	1	...	30·68	30·50	31	38	38	31	...	...	Fair	Fine	SW.
7	30·52	44	51·00	80	W.	...	......	...	1	1	...	1	1	...	30·50	30·30	40	46	46	41·5	...	...	Cloudy	Cloudy	Calm
8	30·66	41	......	75	N.	...	......	1	...	1	...	...	...	...	30·76	30·50	35	44	36	33	...	...	Fair	Fine	Calm
9	30·79	37	......	86	N.	·05	......	...	...	1	...	...	1	...	30·89	30·65	36	42	40	36	...	...	Cloudy	Fine	Calm
10	30·80	41	......	77	N.	...	......	...	...	1	...	...	1	...	30·87	30·70	40	40	36	35·5	0·00	...	Cloudy	Cloudy	Calm
☾ 11	30·70	37	......	71	N.	...	......	...	...	1	...	...	...	...	30·72	30·55	38	42	41	37	...	...	Cloudy	Cloudy	Calm
12	30·65	33	......	77	NW.	·05	......	...	...	1	...	...	1	...	30·67	30·50	31	40	39	29	...	...	Cloudy	Fine	W.
13	30·52	37	51·00	74	N.	...	......	1	...	1	...	...	1	...	30·50	30·35	35	40	40	33	...	...	Cloudy	Cloudy	Calm
14	30·36	42	......	74	W.	...	......	...	...	1	...	...	...	1	30·37	30·15	40	44	40	39	...	...	Cloudy	Cloudy	Calm
15	30·23	43	......	72	SW.	·08	·090	...	1	1	...	1	1	1	30·25	30·	40	43	40	41	...	...	Cloudy	Cloudy	Calm
16	29·80	44	......	77	S.	...	·230	...	...	1	...	...	1	1	29·85	29·60	40	44	38	40	...	...	Cloudy	Windy, rain at night	S.
17	29·88	39	......	76	NW.	...	·300	1	...	1	...	1	1	1	29·97	29·60	35	43	37	34	0·15	·24	Fair	Fine	Calm
18	29·45	46	......	82	SW.	·09	·220	1	1	1	...	1	1	1	29·40	29·15	42	46	36	44·5	...	...	Stormy	Stormy, rain *a.m.*	S.
19	29·49	36	......	72	W.	...	·010	1	...	1	...	1	...	1	29·52	29·22	33	38	39	32	...	·12	Fair	Fine, snow *a.m*	S.
20	29·62	39	50·50	74	NW.	...	·010	...	1	1	...	...	1	1	29·63	29·40	39	39	40	38	...	...	Fair	Fine, rain at night	Calm
21	29·72	40	......	70	N.	·08	......	...	...	1	...	1	1	1	29·82	29·65	38	39	37	39	...	·17	Showery	Fine	Calm
22	29·85	38	......	74	N.	...	......	...	...	1	...	1	1	...	30·01	29·85	35	39	35	34	...	...	Cloudy	Fine	E.
23	30·07	38	......	72	NE.	...	......	...	...	1	...	...	1	...	30·20	30·10	34	40	36	31·5	...	...	Cloudy	Fine	Calm
24	30·05	36	......	72	NE.	...	·300	1	1	1	...	1	1	1	30·03	30·	34	39	38	32	0·30	...	Cloudy	Fine	Calm
25	29·68	38	50·10	74	NW.	·10	......	...	...	1	...	1	...	...	29·80	29·55	40	40	34	35·5	...	·18	Fair	Cloudy, rain *a.m.*	Calm
Aver.:	30·138	41·55	50·99	76·7		0·82	1·960	16	8	29	1	13	19	17	30·19	29·92	40	44	40	38·3	1·10	·81			

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

28th FEBRUARY 1825.

XIII. *On the Method of the Least Squares.* *By* J. Ivory, *Esq. M.A. F.R.S.*

[Continued from p. 10.]

IN the last Number of this Journal I have attempted to demonstrate the method of the least squares without having recourse to the doctrine of probabilities. It must be remembered that the demonstration, in whatever way we attempt it, must inevitably contain something vague. But the arguments that have been adduced for preferring the solution by the method of the least squares to every other, appear to be perfectly sufficient and conclusive as far as the nature of the problem will admit. If the proof by the doctrine of probabilities be more formal, it is not on that account more satisfactory; because it involves precarious suppositions which cannot possibly be rigorously exact. But in order to derive the greatest practical utility from a system of equations of condition, it becomes necessary to connect the solution of them with the rules of chance; and we now proceed to treat the problem in this view of it.

1. It is perfectly impossible to ascertain with mathematical precision the probability of the error of an observation. There is even no proof that the probability will in all cases depend precisely in the same manner upon the magnitude of the error. All that can be accomplished in this theory is to adopt the suppositions that are least likely to deviate much from the truth.

Experience shows that great errors occur less frequently than small or middling ones. If we push this principle to the utmost length, the function $\varphi(e)$, which denotes the probability of the error e, will be greatest when $e = 0$, and it will decrease as e increases. Again, abstracting from errors that are affected by a constant cause, and confining our attention to such only as are irregular and fortuitous, the errors $\pm e$ will be equally probable, which requires that

 $\varphi(e)$

$\phi(e) = \phi(-e)$: and, as it is most reasonable to suppose that $\phi(e)$ is a continuous function, at least within certain limits, it follows that $\phi(e^2)$ is the proper expression of the probability of the error e.

All the errors being contained between certain definite limits, suppose $\pm f$; the function $\phi(e^2)$ will be evanescent when e is equal to or greater than $\pm f$. Therefore, strictly speaking, $\phi(e^2)$ must be a discontinuous function, having real values only between the limits $\pm f$, and no values at all beyond the same limits: but this condition will be sufficiently fulfilled if $\phi(e^2)$ be evanescent when e^2 is infinitely great, and have a very small value when $e^2 = f^2$.

The function $\phi(e^2)$ may be regarded as the ordinate of a curve, corresponding to the abscissæ $\pm e$; and the small area $de\,\phi(e^2)$ will denote the probability of an error between the limits e and $e + de$. Wherefore the integral $\int de\,\phi(e^2)$, taken from $e = a$ to $e = b$, will express the probability of an error between the limits a and b. And because all possible errors are contained between $\pm f$, or $\pm \infty$; it follows that $\int de\,\phi(e^2)$, between the same limits, must be equal to unit, the expression of certainty.

These are properties which it is essential that the function expressing the probability of an error must possess. Let us next consider the probability of the simultaneous existence of a number of errors.

2. The several errors e, e', e'', &c., are independent of one another, since they arise from separate observations; their respective probabilities are, $\phi(e^2)$, $\phi(e'^2)$, $\phi(e''^2)$, &c.; wherefore, by the known rules, the probability of their simultaneous existence is the product,

$$\phi(e^2) \,.\, \phi(e'^2) \,.\, \phi(e''^2) \text{ \&c.} \qquad \text{(P).}$$

Now as every factor is always positive, and is evanescent when the error is equal to $\pm \infty$; it follows that the product will have a maximum relatively to every error, and likewise an absolute maximum for certain definite values of all the errors. If the errors be functions of x, y, &c., the equations of the several maxima will be,

$$\left.\begin{aligned} &\frac{1}{\phi(e^2)} \cdot \frac{d\phi(e^2)}{de^2} \cdot \frac{de^2}{dx} + \frac{1}{\phi(e'^2)} \cdot \frac{d\phi(e'^2)}{de'^2} \cdot \frac{de'^2}{dx} + \text{\&c.} = 0, \\ &\frac{1}{\phi(e^2)} \cdot \frac{d\phi(e^2)}{de^2} \cdot \frac{de^2}{dy} + \frac{1}{\phi(e'^2)} \cdot \frac{d\phi(e'^2)}{de'^2} \cdot \frac{de'^2}{dy} + \text{\&c.} = 0, \end{aligned}\right\} \text{(B)}$$

and all these equations together will determine the values of x, y, &c. which correspond to the absolute maximum.

Again, let ψ denote a rational and integral function of e^2, e'^2, e''^2, &c., consisting of positive terms only. The function will

will therefore be always positive, and it will become infinitely great whenever any of the errors is equal to $\pm \infty$. There will therefore be a minimum relatively to every error, and an absolute minimum for certain definite values of all the errors. The equations of the several minima are respectively,

$$\frac{d\psi}{de^2} \cdot \frac{de^2}{dx} + \frac{d\psi}{de'^2} \cdot \frac{de'^2}{dx} + \&c. = 0, \qquad \text{(C)}$$

$$\frac{d\psi}{de^2} \cdot \frac{de^2}{dy} + \frac{d\psi}{de'^2} \cdot \frac{de'^2}{dy} + \&c. = 0,$$

and all these equations together determine the particular values of x, y, &c., in the case of the absolute minimum.

Let us now suppose that the most probable values of ψ are the several minima, and consequently that the absolute minimum is the most advantageous, or the most probable, value of all: then, since the probability of ψ is just the same as that of the simultaneous existence of the errors which enter into it, that probability will have the product (P) for its expression. Wherefore, in the supposition we have made, it follows that the minima of one function will take place at the same time with the maxima of the other; and hence we get these formulæ, viz.

$$-\frac{1}{\varphi(e^2)} \cdot \frac{d.\varphi(e^2)}{de^2} = h^2 \cdot \frac{d\psi}{de^2},$$

$$-\frac{1}{\varphi(e'^2)} \cdot \frac{d.\varphi(e'^2)}{de'^2} = h^2 \cdot \frac{d\psi}{de'^2},$$

&c.

which render the equations (B) and (C) identical, all the latter being first multiplied by the arbitrary quantity h^2. Now it is manifest that these formulæ cannot be satisfied unless ψ have this form, viz.

$$\psi = f(e^2) + f(e'^2) + f(e''^2) + \&c.;$$

in which case all the formulæ are contained in one, viz.

$$-\frac{1}{\varphi(e^2)} \cdot \frac{d\varphi(e^2)}{de^2} = h^2 \cdot \frac{df(e^2)}{de^2},$$

which determines the function φ.

If the probabilities of the several errors e, e', e'', &c. be expressed by different functions, viz. $\varphi(e^2)$, $\varphi'(e'^2)$, $\varphi''(e''^2)$, &c., it will follow that ψ must have this form, viz.

$$\psi = f(e^2) + f'(e'^2) + f''(e''^2) + \&c.;$$

and then the several formulæ will determine the functions φ, φ', φ'', &c.

3. In order to apply the foregoing reasoning to a system of equations of condition, we must recollect that the most ad-

 vantageous,

vantageous, or the most probable, solution is when the sum of the squares of the errors is a minimum. Hence

$$\psi = e^2 + e'^2 + e''^2 + \&c.;$$

$$\frac{1}{\phi(e^2)} \cdot \frac{d\phi(e^2)}{de^2} = -h^2 \frac{d\psi}{de^2} = -h^2:$$

consequently

$$\phi(e^2) = k\,c^{-h^2e^2},$$

c being the base of the hyperbolic logarithms. It has been shown that the integral

$$\int de\,\phi(e^2) = k\int de\,c^{-h^2e^2},$$

taken between the limits $\pm\infty$, must be equal to unit: and hence

$$k\int de\,c^{-h^2e^2} = \frac{k}{h}\cdot\sqrt{\pi} = 1;$$

wherefore $$k = \frac{h}{\sqrt{\pi}}, \text{ and}$$

$$\phi(e^2) = \frac{h}{\sqrt{\pi}}\,c^{-h^2e^2}.$$

In order to determine h we must employ another consideration. The integral

$$\int e^2 de\,.\,\phi(e^2) = \frac{h}{\sqrt{\pi}}\int e^2\,de\,c^{-h^2e^2},$$

taken between the limits $\pm\infty$, is equal to the sum of the squares of the errors multiplied by their respective probabilities; and it is therefore the limit to which the mean of the sum of the squares of the errors converges as the number of the observations increases. Now the integral is equal to $\frac{1}{2h^2}$; and, if we denote by ε, ε', ε'', &c., the errors of the most advantageous solution, or those of which the sum of the squares is a minimum, we shall have very nearly when the number of observations is great

$$\frac{1}{2h^2} = \frac{\varepsilon^2 + \varepsilon'^2 + \varepsilon''^2 + \&c.}{n},$$

n being the number of the errors. Hence, employing the summatorial prefix S, we get,

$$h = \sqrt{\frac{n}{2S.\varepsilon^2}}.$$

Thus every thing is known in the function expressing the probability of an error. The quantity h may be considered as measuring the precision of the observations. For $\frac{1}{h^2}$ is proportional to $\frac{S.\varepsilon^2}{n}$; and as the latter quantity is independent

of

of n when the number of the observations is great, it follows that $\frac{1}{h'}$ will be greater or less according as the errors, taken upon the whole, are more or less considerable; that is, according as the observations are less or more exact.

4. Let us now consider this system of equations of condition, viz.

$$\begin{aligned} e &= a\,x + b\,y + c\,z - m \\ e' &= a'\,x + b'y + c'z - m' \\ e'' &= a''x + b''y + c''z - m'' \\ &\&c. \end{aligned}$$

the quantities x, y, z, as well as e, e', e'', &c., being indeterminate. Put ε, ε', ε'', &c. for the particular values of e, e', e'', &c. in the most advantageous solution, or when the sum of the squares is a minimum; and let A, B, C be the corresponding values of x, y, z,: we shall have

$$\begin{aligned} \varepsilon &= a\,\mathrm{A} + b\,\mathrm{B} + c\,\mathrm{C} - m \\ \varepsilon' &= a'\,\mathrm{A} + b'\,\mathrm{B} + c'\,\mathrm{C} - m' \\ \varepsilon'' &= a''\mathrm{A} + b''\mathrm{B} + c''\mathrm{C} - m'' \\ &\&c. \end{aligned}$$

and A, B, C will be found from the equations of the minima, viz.

$$\begin{aligned} a\,\varepsilon + a'\varepsilon' + a''\varepsilon'' + \&c. &= 0 \\ b\,\varepsilon + b'\varepsilon' + b''\varepsilon'' + \&c. &= 0 \\ c\,\varepsilon + c'\varepsilon' + c''\varepsilon'' + \&c. &= 0. \end{aligned}$$

Again, let
$$\begin{aligned} x &= \mathrm{A} + u \\ y &= \mathrm{B} + v \\ z &= \mathrm{C} + w \end{aligned}$$

and we may regard u, v, w as the respective errors of A, B, C. Substitute now the expressions of x, y, z, in the values of e, e', e'', &c., and we shall have

$$\begin{aligned} e &= \varepsilon + a\,u + b\,v + c\,w \\ e' &= \varepsilon' + a'u + b'v + c'w \\ e'' &= \varepsilon'' + a''u + b''v + c''w \\ &\&c. \end{aligned}$$

Further, put
$$\begin{aligned} \lambda = {} & (a\,u + b\,v + c\,w)^2 \\ & + (a'u + b'v + c'w)^2 \\ & + (a''u + b''v + c''w)^2 \\ & + \&c. \end{aligned}$$

then square the values of e, e', e'', &c.; add the results into one sum; and leave out the terms which, on account of the equations of the minima, are equal to zero; we shall get

$$\mathrm{S}\,.\,e^2 = \mathrm{S}\,.\,\varepsilon^2 + \lambda.$$

Now the probability of the function $\mathrm{S}\,.\,e^2$ is equal to that of the simultaneous existence of the errors whose squares are added

added together: it is therefore proportional to the product (P), or to the exponential quantity

$$c^{-h^2 S.e^2} = c^{-h^2 S.\epsilon^2 - h^2 \lambda}.$$

Wherefore leaving out the constant factor, the probability of $S.e^2$, or, which is the same thing, the probability of the simultaneous existence of the errors u, v, w, is proportional to

$$c^{-h^2 \lambda}.$$

In order to find the separate probability of the error u, we must take the sum of all the values of the foregoing expression that arise by combining u with every possible value of v and w: it is therefore proportional to the fluent,

$$\int dv\, dw\, c^{-h^2 \lambda},$$

both the integrations being executed between the limit $\pm \infty$.

To determine the integral, expand the squares in the value of λ, and collect the like terms; then

$$\lambda = u^2 S.a^2 + v^2 S.b^2 + w^2 S.c^2$$
$$+ 2uv S.ab + 2uw S.ac + 2vw S.bc.$$

Again, assume

$$t = P u,$$
$$t' = P'u + Q v,$$
$$t'' = P''u + Q'v + R w,$$

and determine the arbitrary coefficients so as to satisfy the condition

$$\lambda = t^2 + t'^2 + t''^2.$$

By equating the coefficients of the like terms of this equation, we shall get

$$P^2 + P'^2 + P''^2 = S.a^2, \qquad P'Q + P''Q' = S.ab,$$
$$Q^2 + Q'^2 = S.b^2, \qquad P''R = S.ac,$$
$$R^2 = S.c^2, \qquad Q'R = S.bc.$$

Hence we obtain $P^2 = \frac{M}{N}$; the values of M and N being as follows, viz.

$$M = S.a^2 \times S.b^2 \times S.c^2 + 2S.ab \times S.ac \times S.bc$$
$$- S.a^2 \times (S.bc)^2 - S.b^2 \times (S.ac)^2 - S.c^2 \times (S.ab)^2.$$
$$N = S.b^2 \times S.c^2 - (S.bc)^2:$$

and it is easy to prove that M and N will be always positive. It will not be necessary to determine the other coefficients. We shall now have

$$\int dv\, dw\, c^{-h^2\lambda} = \frac{1}{QR} \cdot \int dt'\, dt''\, c^{-h^2 t^2 - h^2 t'^2 - h^2 t''^2},$$

the limits of the integrations being the same as before. Wherefore the probability of the error u is proportional to

$$\frac{c^{-h^2 t^2}}{QR} \times \int dt'\, c^{-h^2 t'^2} \times \int dt''\, c^{-h^2 t''^2}; \quad \text{and}$$

and as the integrations produce constant quantities only, the same probability will be equal to the expression,

$$k\,c^{-h^2t^2} = k\,c^{-h^2P^2u^2}.$$

The constant k will be determined by observing that the integral

$$k\int du\,c^{-h^2P^2u^2},$$

taken between the limits $\pm\infty$, comprehends every possible error, and it must therefore be equal to unit. Hence $\frac{k\sqrt{\pi}}{hP} = 1$; and $k = \frac{hP}{\sqrt{\pi}}$. Wherefore, finally, the probability of the error u, in the value A found by the method of the least squares, is equal to

$$\frac{hP}{\sqrt{\pi}}\,c^{-h^2P^2u^2}.$$

If we compare this expression with the error of an original observation, it will appear that the precision of A, the value of the element found by the method of the least squares, is to the precision of the actual observations, as hP to h, or as P to 1. The probability that the true value of the element is between the limits $A(1\pm\delta)$ is equal to the integral

$$\frac{hP}{\sqrt{\pi}}\int du\,c^{-h^2P^2u^2}$$

taken between the limits $\pm A\delta$.

It is easy to transfer what has been proved with respect to u, the error of A, to v and w, the errors of B and C. As the solution we have given extends to three elements, it will necessarily comprehend the subordinate cases of one and two elements; and there is no difficulty, except the length of the operations, of applying the same analysis to any number of elements.

It is not my intention to treat of the practical details of this Theory, but merely to lay before the reader that particular view of its principles which appears most natural and philosophical. All that part connected with the doctrine of chance, is founded on the hypothesis that in all cases the probability of an error depends precisely in the same way on the magnitude of the error, or that it is always the same function of the error. Now, I believe, it will be allowed that the grounds of this supposition are much less sure than the evidence adduced in proof of the method of the least squares. There would therefore be a great logical fault in making the most advantageous solution of a system of equations of condition

dition depend upon the arbitrary expression of the probability of an error. But when we set out with demonstrating the most advantageous solution from the nature of the equations of conditions, the whole theory follows naturally, and is placed on its proper foundation.

For better illustrating the principles of this important speculation, I shall resume the subject on a future occasion, and offer some further remarks upon it.

February 3, 1825. JAMES IVORY.

XIV. *On the Marmolite of Mr.* NUTTALL. *By* LARDNER VANUXEM*.

THE description and analysis of this mineral was published by Mr. Thomas Nuttall, in vol. iv. No. 1. of the American Journal of Science and Arts.

Having last summer visited with Professor Keating the Hoboken locality of serpentine †, I was enabled to make a number of observations; the communication of which I hope will not be uninteresting, or considered unimportant by the Academy.

For some years past considerable doubt has prevailed among many of the best mineralogists with respect to the propriety of retaining serpentine as a mineral species, having few or none of those external characters required to substantiate its claim to such a rank. By some, serpentine has been considered as a rock, whose substance appeared to be the result rather of a mixture of different minerals, or the elements of different minerals, melted and deposited in a confused state, than an homogeneous substance or simple mineral whose aggregate has been effected by the power of chemical affinity. That it is not generally regarded to be a mineral, *sui generis*, it

* From Jour. of the Acad. of Nat. Sciences of Philadelphia, vol. iii. p. 129.

† The author of the paper on the marmolite considers the serpentine of Hoboken, (N. J.) to "appertain rather to the transition than the primitive range;" on what ground I know not, further than the circumstance of a part of it being in fragments, and these fragments connected together so as to form a breccia: but this fact is susceptible of an explanation by which the primordial character of the mass remains in all its integrity. Serpentine, like many other rocks, is split or cracked in various directions; in some parts of the serpentine of Hoboken, the cracks or fissures are very numerous, of course the fragments are small. These fissures in many instances are filled with carbonate of lime, so that the fragments form one solid mass. Now this fact is analogous to those parts of a rock, one of the primitive class for example, which are traversed in different directions by veins in series; and no one ever supposed that the intervening masses lost their primitive character from the presence of such veins.

it will be sufficient for my purpose to quote two authorities, Haüy and Mr. Brochant: the former considered it as a rock, and hence it has no place in his mineralogical method; and by the latter it was (improperly) arranged (1819) as a subspecies of talc, under the name of *Talc esquilleux*, i. e. scaly or rather splintery Talc.

From the difference of opinion with respect to the rank and place which serpentine ought to hold in our mineralogical systems, it is really a desideratum that all obscurity upon this subject should be removed, serpentine being important from its great abundance, and the many uses to which it is and may be applied. The object of this communication is to make known the real character of serpentine, which the writer believes he has ascertained, and submits his views with all due deference to the Academy.

It is to Mr. Nuttall, whose zeal and talents as an observer of nature are well known, and whose contributions to natural history have already been so respectable, that we owe the introduction of a mineral to our notice, which in my opinion throws such light upon this hitherto obscure subject as to enable us to assign to serpentine its proper place in the systems of mineralogy, and thereby to remove a part of the confusion existing in that almost, as we might say, chaotic science.

The marmolite, the mineral alluded to, possesses those external characters which are acknowledged by all mineralogists to be typical of a species; and is moreover uniform in its composition, as ascertained by the analysis of specimens from two localities that are nearly two hundred miles from each other; and has the same analogy to serpentine that all the lamellar or crystalline minerals have to their compact varieties.

The description which Mr. Nuttall has given of marmolite is as follows: "The texture is foliated, with the laminæ thin, and often parallel, as in diallage. Sometimes also cleaving in two directions parallel to the sides of an oblique and compressed four-sided prism. These laminæ, sometimes a quarter of an inch broad, are commonly collected into radiating or diverging clusters, of a pale green or greenish gray colour and a pearly submetallic lustre, soft enough to be easily cut by a knife, and almost perfectly opaque, inflexible, and brittle. Its powder is unctuous and shining. By the influence of the weather it becomes whitish and more brittle. Its specific gravity by Nicholson's balance was 2·470."

"*Chemical Characters.*—Before the blowpipe it decrepitates, hardens, and slightly exfoliates without showing any signs of fusion."

To the above description I have but a few observations to make; 1st. That no notice is taken of the cross fracture of the marmolite, which, in the fresh specimens, is important, from its identity to the precious serpentine, *i. e.* compact, and presenting those minute scales or splinters which result from small portions in part detached from the mass, exhibiting the appearance of wax when broken.—2dly. I was not able, in any of the specimens in my possession, either in those from Hoboken or Bare Hills, to discover natural joints in more than one direction, namely, that which gives the lamellar structure to it.—3dly. With respect to the opacity of the mineral, it is true that all those specimens are opaque in mass which have been exposed for a long time to the action of the atmosphere; but these if put into water become transparent, and the fresh specimens possess a considerable degree of translucency.

Of the Composition of the Marmolite.—I was induced to undertake the analysis of the Bare Hill variety, from a desire of knowing the proportion of each of its constituents, the quantity of silex only being given by Mr. Nuttall: and finding a difference between his result and my own, I was led to examine that also of Hoboken, particularly as Mr. Nuttall had a loss of 2½ per cent.; he omitting to deduct from the 46 parts of magnesia the 2 parts of lime which he found in it.

I should offer my result with diffidence, if I had not assured myself by repeated analysis of the exact quantity of silex, water and iron, in each of them. I did not find any lime in the Hoboken mineral, though I tried by one of our most delicate tests—namely, oxalate of ammonia. I made no search for chrome, the discovery of atoms being of no consequence in the point in question.

The *modus operandi* was to calcine a portion for water; to digest another portion with nitro-muriatic acid until every particle was attacked; then to evaporate to dryness by a gentle heat, so as not to decompose the salts of iron and magnesia in setting the silex free; to dissolve the salts with acidulated water, and filter. The iron was separated from the liquor by succinate of ammonia; and the magnesia, as the residue, was obtained by evaporating the liquor, and then calcining.

In the same manner I also analysed the beautiful precious serpentine of Newburyport (Mass.), wishing to know if in its composition it accorded with the European specimens; and also to confirm my opinion of the chemical identity of the marmolite and serpentine. The results of these experiments are as follows:

Silex

	Bare Hills.	Hoboken.	Precious Serpentine.
Silex	42·69	40	42
Magnesia . . .	40	42	40
Water . . .	16·11	16·45	14·38
Deutoxide of iron	1·16	·90	1
Loss	·4	·65	2·62
	100	100	100

By comparing the above analyses with the analyses of the precious and all the hydrous serpentines, no real difference can be perceived; hence we must conclude them all in that respect to be the same. Mr. Nuttall, on the ground of chemical composition, is of opinion that marmolite might equally "be referred to talc or steatite." Now all well characterized talc contains much more silex, less magnesia, and less water, in the proportion of about 62·5 silex, 31·5 magnesia, and 6 water. As to steatite, I can only consider it as a rock of which talc is the basis; and hence its composition may vary by admixture with other rocks or minerals, as is the case with every great mass of mineral matter. I have no doubt that among the many analyses we have of the substances arranged under steatite the marmolite may be found, and probably even also among the diallage, bronzites, &c., though in an impure state.

To conclude: The marmolite corresponds with serpentine in all its important characters; to wit, composition, infusibility, hardness, and specific gravity: also in those characters of less importance; as colour, fracture not dependent upon internal arrangement of particles, lustre of the same, &c.; and differs only as to crystalline structure, and lustre thereon depending; both of which circumstances belong to every mineral species which present crystallized and compact varieties.

All mineral substances are identical that agree in composition, hardness, specific gravity, and primitive form or crystallization;—but the absence of the latter character, so far from making a specific difference, is merely considered in the light of an accident. Conceiving then the identity of the marmolite and serpentine to be fully proved, we shall have three sub-species or varieties of this mineral, according to the idea or importance attached to these sub or minor distinctions.

1st	Sub-species.	Marmolite or lamellar Serpentine, ...	rare, forms masses not exceeding a few inches in diameter.
2d	Do.	Precious or compact translucent Serpentine...............	more abundant; but if it forms rocks, they are of very limited extent.
3d	Do.	Common or Serpentine rock..........	forms rocks of great magnitude, often very impure.

XV. *An*

XV. *An Account of the Earthquakes which occurred in Sicily in March* 1823. *By Sig. Abate* Ferrara, *Professor of Natural Philosophy in the University of Catania, &c. &c.**

On Wednesday the 5th of March 1823, at 26 minutes after 5 P. M., Sicily suffered a violent shock of an earthquake. I was standing in the large plain before the palace, in a situation where I was enabled to preserve that tranquillity of mind necessary for observation. The first shock was indistinct, but tending from below upwards; the second was undulatory, but more vigorous, as though a new impulse had been added to the first, doubling its force; the third was less strong, but of the same nature; a new exertion of the force rendered the fourth equal on the whole to the second; the fifth, like the first, had an evident tendency upwards. Their duration was between sixteen and seventeen seconds; the time was precisely marked by the seconds hand of a watch which I had with me. The direction was from north-east to south-west. Many persons who ran towards me from the south-west at the time of this terrible phænomenon were opposed by the resistance of the earth. The spear of the vane on the top of the new gate connected with the palace, and upon which I fixed my eyes, bowed in that direction, and remained so until the Sunday, when it fell; it was inclined to the south-west in an angle of 20°. The waters in the great basin of the botanical garden, as was told me by an eye-witness, were urged up in the same direction by the second shock; and a palm-tree thirty feet high, in the same garden, was seen to bow its long leafless branches alternately to the north-east and south-west, almost to the ground. The clocks in the observatory, which vibrated from north to south and from east to west, were stopped, because the direction of the shock cut obliquely the plane of their respective vibrations; and the weight of one of them broke its crystal. But two small clocks in my chamber kept their motion, as their vibrations were in the direction of the shock. The mercury in the sismometer† preserved in the observatory was put into violent motion, and at the fifth shock it seemed as much agitated as if it were boiling.

To the west of Palermo, within the mountains, the earthquake retained little of its power; since at Morreale, four miles distant, trifling injury only was sustained by the (Benedictine) monastery of S. Castrense, the house of the P. P. Conviventi, and the seminary of the clergy. At Parco, six miles distant,

* From the Boston Journal of Philosophy and the Arts for Sept. 1824.

† An instrument, apparently, for the purpose of showing the violence of the shock of an earthquake.—Tr.

Mary's

Mary's college, the monastery, the parish church, and a few peasants' cottages, were all that suffered. At Piana, the battlements of the tower were thrown down. But more of its power was felt in places on the sea-coast, as appears from its effects at Capaci, four miles distant, where the cathedral and several houses were ruined; and at Torretta, fourteen miles off, where the cathedral, two storehouses and some dwelling-houses were destroyed. Beyond, its power continued to diminish; and at Castellamare, twenty-four miles off, the state-house only had the cleft, which was made in 1819, enlarged.

In maritime places east of Palermo the shock was immense. At Altavilla, fourteen miles from Palermo, the bridge was shaken. At Trabia, twenty-one miles, the castle, and at Godiano, the cathedral and some houses, were destroyed,—enormous masses from Bisambra, a neighbouring hill, were loosened, and fell. At Termini, twenty-four miles, the shocks were very violent, exceeding all that had happened within the memory of its inhabitants. Those of 1818–19 were very strong, but the city received at those times no injury; now, the convent of St. Antonio, Mary's college, and various private houses felt its effects.

The warm waters, as well those of the baths as those from the neighbouring wells, which proceed from the same subterranean source in the mountains along the coast of Termini, increased in quantity and warmth, and became turbid; consequences that always succeed convulsions of the earth, by which their internal streams are disordered. The clay tinged the fluid with its own colour, and equal volumes of the water yielded a greater quantity of the clay than before, when the colour was deeper*. Most of the houses in the little new town of Sarcari, two miles from the shore, and consisting of less than a hundred houses, were rendered uninhabitable; the walls were thrown down, and the more lofty buildings were all damaged. The effects of the earthquake are found to be greater in proportion to its advance eastward.

Forty-eight miles from Palermo, at Cefalu, a large city on the shore of a promontory, the effects were various and injurious. Without the walls, two convents, a storehouse, and some country-houses, were injured, but no lives were lost. The sea made a violent and sudden rush to the shore, carrying with it a large ship laden with oil; and when the wave retired

* The warm and mineral waters of St. Euphemia, in Calabria, which sprang up after the memorable earthquakes in 1638, presented the same phænomena in those of 1783.—*Grimaldi Descr. dei Trem. del* 1783.

[See also the remarks on volcanic phænomena in M. de Humboldt's paper on the *Rio Vinagre* in our present number.—Edit.]

tired she was left quite dry; but a second wave returned with such immense force that the ship was dashed in pieces and the oil lost. Boats which were approaching the shore were borne rapidly forward to the land; but at the return of the water they were carried as rapidly back, far beyond their first situation. The same motion of the sea, but less violent, was observed all along the shore, as far even as Palermo. Pollina, a town with nine hundred inhabitants, occupying an elevated position at a little distance from the sea, was injured in almost every building; particularly in the church of St. Peter and Nunciata, in the castle, the tower, and in other places. Nor did Finale, a little nearer the shore, suffer less; five of its houses fell in consequence on the 11th of March.

Beyond the towns which have been mentioned, towards the interior of the island, the shock was vigorous to a certain extent; but kept decreasing as it proceeded, throughout the whole surface. At Ciminna, south of Termini, a statue was shaken from its place on the top of a belfry in front of the great church, and a part of the clock-tower falling, killed one person, and badly wounded another. In Cerda, the shock affected the great church, some houses, and half of one of the three forts placed near the city to support the earth on the side of a great declivity.

The only church in Roccapalomba, which is situated at the top of an acclivity was ruined. The parish church and some private houses in the little town of Scillato were overthrown. In Gratteri, a large town south of Cefalu, injury was sustained by the church of St. James, and other houses. Considerable damage was sustained by various churches and many private houses in Colesano, a town containing two thousand inhabitants, and situated on an inclined plain on the eastern side of the mountains of Madonie. One of the colleges de Maria was rendered uninhabitable. The hospital, a grand fabric, was made a heap of ruins. The loss is calculated at about thirty thousand *onze*. In the vicinity of Pozzillo and St. Agata, through a large extent of land many long fissures and caverns were made. Similar caverns and fissures in argillaceous chalk were opened near the little town of Ogliastro, sixteen miles south-east of Palermo. At Isnello, at the foot of the Madonie mountains, the injuries which were received in 1819 were increased: Geraci, among the same mountains, suffered a like fortune in the ruin of the cathedral: Costelbuono and St. Mauro, within the same regions, were damaged, both by the former and by the last convulsions; by the last, the cathedral, the church of St. Mauro, and five private houses suffered much.

The

The damage done to Castelbuono is reckoned at twenty-two thousand *onze.*

The northern coast of Sicily, towards Cape Cefalu, after bending to form the eastern part of the great bay included on the west by the mountains to the left of Palermo, extends into the sea towards Eolie (the Lipari islands), and presents, towards them, a hollow front, the western part of which is formed by Cape Orlando, and the eastern by Cape Calava. Places situated about this bay suffered the most violent convulsions. Nato, containing four thousand souls, and situated on an elevation, was almost entirely laid waste, and a great number of private houses destroyed; the monastery, hospital, the churches of St. Peter, Anime del Purgatorio, St. Demetrius, and the cathedral, were in a great measure overthrown. The Quartiere del Salvadore suffered less: a transverse cleft was made in the earth, and fears were entertained lest the whole elevation upon which the city is built should be overthrown. Only two persons lost their lives; for the people, warned by a slight shock which was felt some hours before, had all fled into the country. Directly in front of Vulcano, (one of the Æolian isles,) Patti, a city built on the declivity of a mountain, and at the distance of half a mile from the eastern extremity of Cape Calava, had its cathedral, bishop's palace, convents, and many private houses injured. With the copious showers of the fifth fell some roofs; various houses in the country were ruined. Pozzodigotto, Meri, and Barcellona were injured a little. At Barcellona a wide cleft was made in the belfry of the church, and threatened its ruin. The shock at Milazzo on the sea was violent, as also at St. Lucia, six miles from it, situated on an eminence; but without any bad consequences. Some damage was done to the hospital, several churches, and private houses, at Messina. In the interior of Sicily the motion was communicated as if it were far from the centre of force: in some places towards the south some buildings which were old and out of repair felt the effects, particularly at Caltauturo; and at Alimena, in the cathedral and convent of the Reformed. The shock gradually wasted itself as it advanced; and at Catania so slight was the impression made on the people, that they went to the theatre the same evening. It was perceived by a few persons only in Syracuse and in some of the neighbouring towns. In the district of Modica, towards Cape Passaro, scarcely one felt it. No bad effects were produced by it in the southern parts of the island: in the western it was felt, but without injury. It was pretty strong at Alcamo, but slight at Trapani.

Injuries at Palermo.—The ancient city of Palermo was founded

founded upon a rocky tongue of land between two large and deep bays. The extremity of this point constitutes at this day the centre of the modern city. Matter, transported thither by the water from the interior, and thrown up by the sea, together with the labour of men, has gradually filled up the lateral spaces, and extended the peninsula with this transported and alluvial earth, and formed the present soil. It is now composed in part of calcareous rock, and in part of mud or alluvial earth; both are traversed by canals and large conduits for the circulation of water for common use, and by common sewers communicating with the neighbouring shore. The adjacent parts present a surface composed of calcareous tufa, and an earthy aggregate tender and friable; but deeper down it is more durable, and partly siliceous. The cheapness of the tufa and the ease with which it is wrought have caused its adoption as a building stone, contrary to the custom of our ancestors, as appears from the immense excavations and pits about Syracuse, Girgenti, and some others of the ancient cities of Sicily. Till lately, the common cement was composed of a fat earth, to which ashes were sometimes added; it was called *tajo*. Within a few years, lime and sand have been used. But they do not always employ for lime the stone which is hardest and most proper, nor that which requires an equal degree of heat in calcination; nor are all the pieces white. It is not slaked methodically, nor mingled with that patience which caused the ancients to say that lime should be tempered by the sweat of the brow. And here, indeed, this labour is the more indispensable, as Palermo is destitute of puzzolana, and of those ferruginous earths which render such valuable service to those volcanic towns of the island which can obtain a cement so adhesive and durable.

The soft rock of the surface serves in large masses for a foundation upon the clay. But the brittleness of the rock, and the instability of the earth, its readiness to change from a level at the least motion, or by the action of moisture, which the air and soil of Palermo make permanent, render the foundation very far from firm. I have seen pieces of the foundation of large edifices so entirely reduced to earth as to be removed with a spade. This inconvenience exists even when the rock in its natural situation serves as the base. Where a building is raised upon a soil the parts of which are of different natures, it must suffer much from the unequal resistance of this soil. The right side of the royal palace has for several years been inclining from a perpendicular, in consequence of its having been placed on the ancient alluvial formation, while the remainder of the building rests on a rock. Sometimes we see buildings

buildings raised on an inclined plane, with one part of the base more elevated than the other: in this case it is evident that the oblique pressure is compounded of two forces; one perpendicular to the resistance, and which is overcome by it; the other parallel with the resistance, but which, not entering into the action, operates in its own direction. The equilibrium is thus destroyed, and the stability of such buildings cannot be of long duration.

Our author goes on to speak of the necessity of having acute angles to many of the streets on account of their crookedness, and how liable buildings are, from this circumstance, to be thrown down; that regular foundations are not very much used, and even when used are soon destroyed by the action of the atmosphere, by water, and many other causes. He finds fault with the forms of the stones used in building, with the cement, its want of adhesion; and compares houses constructed in this manner with those of ancient Tyndaris, many of the walls of which, standing on the top of some of the highest mountains, were so well balanced, the pieces so nicely cut and jointed, even without any cement at all, that they have stood firm for a thousand years.

Upon foundations so infirm, and with materials so frail, buildings are raised to the height of four or five stories.—He next remarks on the disproportion of the thickness of the walls to the weights they sustain. Though diminishing exceedingly in thickness from bottom to top, they are still very much weakened by the great number of windows, are overburdened by immense cornices, and little chambers, and kitchens, projecting fearfully beyond the sides; and by terraces and balconies loaded with enormous vases of stone. The beams which support the floors scarcely touch upon the walls, are not charred nor faced with lead to defend them against the moisture, and are almost always injured by the lime in which they lie. Many particulars of this kind our author has mentioned, all tending to show the great want of prudence in the manner of building.

In the night of the 1st of September 1726, continues Professor Ferrara, an earthquake destroyed, or very much injured, all the buildings situated on the muddy soil, and many (which were out of repair or badly constructed) placed on rock. Earth, of the nature of the first, is less capable of receiving motion from a shock than the last, since it possesses less resistance. But facts show that this advantage is more than compensated by want of stability in edifices raised upon it.

 At

At Messina, in 1783, all the buildings upon a plain, and upon earth thrown up by the sea, were destroyed; while those on the neighbouring hills were not moved. The same happened at Calabria, and in 1805 in the district of Molise. In this account we should notice the cavities made in the earth. They were esteemed by the ancients as preservatives against earthquakes,—not by affording an outlet to the subterranean vapours, as some have thought, but by interrupting or diminishing the course of the shock.

The houses were rebuilt in the same situation, and after the same mode; the fissures of those which were damaged were, as we now observe them, only covered over on the outside by a slight coating of lime. These very places, and precisely the same houses, were this year laid waste; and so they will always be in future, unless a more prudent and more reasonable method shall regulate new buildings and new repairs.

Professor Ferrara proceeds to give a very particular account of the effects of the shock upon buildings in different situations, which it would be hardly interesting to repeat here. Most of the injury, he says, was done by the second impulse of the shock, when the spear of the vane on the new gate was bent, and the water in the basin in the botanical garden was forced violently up one side. Immediately after the shock, he remarks, the apparent injuries were not very great: but the blow was given, and the long and abundant showers of rain which succeeded continued to develop and increase the injuries; and now, though not very many buildings are entirely destroyed, yet there is scarcely one which has not received some damage. Here follow some notices of the dreadful consequences which befel many of the inhabitants, from the falling of the timbers and stones and walls; of the vases from the piazzas into the streets; and many other things which it is unnecessary to mention more particularly. Nineteen persons were killed and twenty-five wounded. In the earthquake of Sept. 1, 1726, four hundred were killed and very many wounded.

In the close of this chapter he remarks, Do not these sad facts impress us with the necessity of every attention in the construction of new edifices? Already have the zeal of the governor, the facilities offered by the senate, and the concern of the active citizens, given a strong impulse to the reparation of the disasters. Soon will the shadow of the past calamity pass away, and the grand city of Palermo will be still more beautiful. When we reflect upon the immense list of earthquakes which Sicily has suffered, and the possibility of its

its increasing every moment, we feel the inevitable necessity of holding ourselves strongly prepared to meet the sudden assaults of so powerful an enemy. Messina, which suffered so much in 1783, although violently moved by this last shock, experienced from it no bad effects; for this noble city has risen from her ancient ruins robust and majestic. Catania, in 1818, was convulsed in a terrible manner; but its inhabitants were enabled to contemplate without a tear all the little injury sustained by their beautiful fabrics*.

Succeeding Shocks.

After the shock of the 5th, the black clouds which covered the heavens on the north and west formed a dark band, measuring from the zenith towards the horizon 60°, and extending from north to south. It was terminated at the base by a circular line passing from north to south, through the west, and elevated at the southern part about 30° above the horizon. The sky itself was very clear, and its extreme brightness was increased by the contrast with the dark band above, and by the sun just on the point of setting. A little below the band were two other lines parallel and perfectly regular. This mysterious appearance inspired with fear the minds of the people, who are always seeking in the heavens for signs of future events. But it preceded a tempestuous night which followed with torrents of rain, with thunder, snow, hail, and wind†.

* After the fatal earthquake of 1693, in Catania, by which eighteen thousand persons perished, the people began to build of one story, and always after the plan of barracks. But as the fear passed from their minds they raised their houses two stories, and sometimes even three, and not with much solidity. Since the middle of the last century, the excellent materials supplied them by Etna, the good method and prudent regulation of the stories, have promised long duration to this city. It may possibly be injured, but cannot be easily ruined, although at the foot of the most formidable volcano in the world. After the catastrophe of the 5th of March in Palermo, the lieutenant, the pretor, senators, and police, exerted all their zeal. They obliged proprietors to prop up their houses within twenty-four hours, or to demolish them if they were not susceptible of propping. The senate took upon themselves the charge of repairing the houses of poor proprietors, together with the expenses.

† In all times signs have been mentioned as announcing earthquakes near at hand. People read them in the air and upon the earth; and some philosophers even have given them credence. The frequent occurrence of these signs, without the expected phænomena, is a sufficient argument against them. But less uncertain are those which accompany the phænomena, as rain and thunder. To that of 1693 such fearful storms succeeded, that for many hours, at Catania, the groans and voices of the miserable wretches buried under the ruins were drowned by the roaring of the torrents of rain and the tremendous thunder. The same circumstances took place at Calabria in 1783; and we were witnesses of the same on the night of the 5th of March. An extraordinary quantity of electric fluid is developed, and being

On the night of the 6th, at forty-five minutes past one, in St. Lucia de Millazzo, six miles from the shore which looks towards Volcano and Stromboli, a severe shock was felt; and afterwards, at various intervals, horrible noises were heard, four distinct times, rumbling fearfully beneath them; and finally, at half past three o'clock, the shock was repeated. Both were felt at Messina, but without any subterranean noises. Nothing of it was felt at Palermo, or in any places in the west. At fifty-six minutes past ten, in the night of the 7th, another shock was felt at Palermo, sufficiently strong to put in motion the pendulum of a small clock which I had stopped that I might regulate it in the morning. Its vibration from N.E. to S.W. showed me with certainty the direction of the shock. Light ones were felt on the 26th. On the 31st, at two and fifty-two minutes P.M. one was felt at Messina, moderately severe, of five or six seconds duration, and undulating. Two others on the 1st of April, and one at Castelbuono on the 28th. I should add that they mention a slight one there on the 16th of February; but they are more certain of those of the 5th of March; one at 1 P.M., the other at 3. These were they which induced the inhabitants of Naso to leave their habitations and flee into the country, where they were when their city was laid waste. Here the Professor mentions many other places in which small shocks were felt in July and August: but as no important remarks are made, we pass over them to his more interesting chapter of physical observations.

[To be continued.]

conducted from the deep cavities of the earth to the surface by the force of equilibrium, produces there extraordinary vaporization when hygrometers have shown extreme dryness. The atmosphere, charged beyond measure with vapours, will give room to their decomposition, which changes them into vesicles and then into rain. Fiery meteors will be produced by the electric fluid, liberated by the passage of the vapours into water. If hydrogen gas escape from the earth, it may be inflamed by the electric spark, and present the appearance of fires. I should mention here, that in volcanic regions signs may sometimes precede earthquakes; but this happens there by the proximity of the place of the subterranean operations to the surface of the earth, which circumstance connects the internal phænomena with those of the adjacent atmosphere. On the morning of the 8th of March 1669, at Pidara, a town on the side of Etna, the air became obscure as by a partial eclipse of the sun; soon after, the earth began to shake, and continued so until the 11th, when an immense fissure opened near Nicolosi, a neighbouring town—a sparkling light appeared over the fissure; and on that very day, while the terrible shocks were levelling Nicolosi with the ground, an enormous burning river, amidst horrid rumblings, roarings, and explosions, was belched out, which flowed fifteen miles, covering a great extent of land, and for four months spreading terror over Sicily.—*Bor. de. Inc. Ætn., Ferr. Descr. dell' Etna.*

XVI.

XVI. *On the Use of Functional Equations in the Elementary Investigations of Geometry.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

A FEW years ago, when the French edition of Legendre's Geometry first fell into my hands, I was considerably attracted by the famous second Note. I bestowed some attention upon the subject, and was led to a particular artifice, which I then conceived sufficient to destroy the force of Mr. Leslie's objection to the legitimacy of the investigations. Accident brought before me, some weeks ago, a volume of your Journal, which recalled my former ideas. It contained two papers upon the subject, signed DIS-IOTA*, which it is needless further to characterize than to attribute to Mr. Ivory. He firmly establishes, I think, the objections which he formerly made (likewise anonymously) in a letter to Mr. Leslie. There cannot remain a doubt that the supposition of the identity of the functions φ, φ', φ'', &c. involved in Legendre's "*mise en equation,*" is fully equivalent to Euclid's axiom. Legendre's method then falls to the ground, in so far as he intended it to overcome this great difficulty of geometry: but it still retains all its interest as a method of investigating the elementary relations of space. We all know the splendour of *analytical* mechanics, compared with the old investigations by means of diagrams; and how pretty would it be, even although useless, were our geometries condensed into a few families of formulæ, each bearing as wide a meaning as the well-known $\Sigma f \delta p = 0$ in mechanics. In this light alone am I inclined to view Legendre's investigations; and I feel a deep interest in attempting to elucidate them. In a few points Mr. Ivory's (or Dis-Iota's) papers appeared to me somewhat deficient. They were in my hands only for an hour or two, so that I cannot now refer to the particular passage which left this impression. I therefore submit my ideas to you in the form of a few general critical remarks. Let us state in the first place the exact nature of Legendre's reasoning respecting the constitution of his functionary equation. He discovers from superposition, that a triangle is determined by the constancy of the three quantities A, B, and c; or, in other words, that these *are all* the variables upon which the value of C can depend. But he asserts, if C required the combination of them *all*—for its determination an analytical absurdity would exist in the equation $C = \varphi(A, B, c)$. The only possible manner then in which C can receive a determinate character from the constancy of these quantities is ex-

* On the Theory of Parallel Lines. Phil. Mag. vol. lxiii. p. 161 & p. 246.

pressed

pressed by the equation $C = \phi(A, B)$, which is therefore true. This is quite logical. It would be a waste of time to examine Mr. Leslie's objections to the universality of the law of homogeneity. They must have escaped him in a moment when his eagerness to put down analysis (which he seems to consider synonymous with mysticism) got the better of his natural quiet apprehension of intellectual truth. They will be found sufficiently discussed in Baron Maurice's paper. But he brings forward an objection of a different character, which probably deserves greater attention than it has met with. He attempts to deduce an absurdity by the employment of similar reasoning in a precisely similar case, and hence infers the fallacy of the whole procedure.

A triangle, says he, is determined by an angle and its including sides; or, these are all the variables which can enter into the determination of the base. But the angle cannot enter in virtue of the law of homogeneity. Hence $c = \phi(a, b)$. Playfair, Legendre, and Maurice, it is well known, have all insisted in reply to this, that there exists a most obvious distinction betwixt the two cases. They have insisted that the different relations in which C and c stand with respect to their respective standards of admeasurement, render necessary the introduction of a specific reasoning suited to each case. It is quite plain that the adoption of any linear unit is altogether conventional, and the unit itself of course variable. A line taken by itself, then, is of no definite magnitude. The right angle, on the other hand, is a fixed and determinate quantity—a quantity quite independent of every other; and consequently every angle *as a part of it* is likewise fixed and determinate. Now let us see what effect this distinction will have upon the similar equations

$$C = \phi(c, A, B)$$
$$c = \psi(C, a, b).$$

There exist two reasons for dismissing c from the first of these; a reason arising from the operation of the law of homogeneity, and a reason arising from its *indeterminateness*. On the other hand C is *a determinate quantity*;—but does that destroy the *heterogeneity* of the latter expression? C is certainly definite, but it is merely so as it is referable to an independent unit. It is but a portion of that unit, and consequently still a quantity *of its own kind*. There is no other angle in ψ with which it may be connected, in order that it bear a relation to the ratios of the lines a, b, c; and the law of homogeneity therefore ordains its dismissal. Even allowing the distinction contended for, Mr. Leslie's *reduction* thus seems perfectly valid. But the geometers I already quoted have not thought it so. If the right angle, says Legendre, be termed 1, all angles

angles will be mere numbers, and of course C as a number may be legitimately determined by

$$C = \psi\left(\frac{a}{b}, \frac{b}{c}\right).$$

It is here strangely forgotten that the equation *right angle* = 1 is merely symbolical or abbreviatory, and can hold only when angles are compared together: whereas C in the foregoing equation is not determined *to be a part of the unit which represents* R, but of the abstract unit which measures the ratio $\frac{R}{R}$. To show the difficulties to which the French geometers seem reduced in their attempts to gloss over this curious piece of reasoning, let us quote one sentence from Maurice's dissertation. After speaking of the trigonometrical equation

$$\cos C = \frac{1 + \frac{b^2}{a^2} - \frac{c^2}{a^2}}{2\frac{b}{a}}$$

as derived from

$$C = \phi(a, b, c)$$

he says: "Now this example is not, as might at first sight be supposed, at variance with the principles here laid down. If the angular unit, though not affecting all the terms, has disappeared, the reason is, that the proposed equation turns out to establish that a certain function of the sides of a triangle is a function of one of its angles: and as this latter function may be transcendental, and consequently equal to the cosine of the arc which corresponds to that angle, it follows that just as we have the general relation angle $= \frac{\text{arc}}{\text{radius}}$, so also we have a certain cosine (and every cosine must be an abstract number) equal to a certain algebraical function of the ratios between the sides." This paragraph has certainly the merit of mystifying the subject considerably. Upon analysing it, however, we find it composed of two assertions. Maurice first states that the functional equation may give a transcendental function of the angle equal to a function of the ratios of the sides; and that this transcendental function may be a number, or, as it is, the cosine. This is singularly at variance with another part of his paper, in which he rightly asserts that no unit can disappear, excepting by division, by itself. Here, however, an angular unit disappears without division; and Maurice accounts for it merely by stating that it does so!

These remarks, brief as they are, will, I hope, sufficiently show the highly objectionable character of the manner in which one party in the controversy would distinguish betwixt the

the two cases. We are not prepared, however, to follow Mr. Leslie in his entire rejection of this application of analysis. It seems to us, on the other hand, that the equation furnished by superposition or experiment ought to contain all the truths which are deduced from the same experiment by geometrical reasoning. We are accordingly rather inclined to search for the cause of the difficulties that have startled us, in some imperfection in the mode of expressing or of treating the equations. That imperfection seems to me to be a deficiency in the original equation as given by Legendre. Superposition does not inform him that the vertical angle is determined by the base and its adjacent angles *alone*,—but merely that it will be constant if they are constant; and hence that it must be determined by these *variables* and CONSTANTS alone. Noting these constants by γ, the original equation becomes

$$C = \phi\,(c, A, B, \gamma)$$

The distinction drawn by Legendre, &c. betwixt lines and angles, will furnish us with the analysis of γ. It must, if any quantity at all, be composed either of constant lines or constant angles. It cannot be composed of constant lines, for there are no such quantities in existence. But there is a constant angle; γ may therefore be a function of the right angle. It is not therefore a linear, but an angular magnitude. This is enough to entitle us to reject the foregoing equation as impossible, and to adopt as the true one

$$C = \phi\,(A, B, \gamma)$$

It is likewise enough to show that C cannot be rejected from Mr. Leslie's equation, which then becomes

$$\frac{C}{\gamma} = \psi\left(\frac{a}{b}, \frac{b}{c}\right)$$

as it ought to be.

If Baron Maurice will return to the subject and contemplate it under this aspect, he will find, I trust, all difficulties dismissed, and instead of the puzzling equations

$$\cos C = \phi.\, C$$

$$\text{and angle} = \frac{\text{arc}}{\text{radius}}$$

he will have

$$\cos C = \phi \frac{C}{\text{right angle}}$$

and $$\frac{\text{angle}}{\text{right angle}} = \frac{\text{arc}}{\text{radius}}$$

expressions in as complete accordance as he could wish with the universal law of homogeneity.

I meant to have taken this opportunity to make some observations

servations on Mr. Leslie's method of investigating the theorem of the composition of forces, which is remarked upon with some severity by Dis-Iota: but I have likely enough already written more than many of your readers will bear with. You shall probably again hear from me.

January 19, 1825. Σ.

XVII. *A new binary Arrangement of the* Brachyurous Crustacea. *By* A. H. Haworth, *Esq., Fellow of the Linnæan and Horticultural Societies of London, and of the Imperial Natural History Society of Moscow, &c. &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SINCE you published the natural distribution of animated nature, on a new binary or dichotomous plan, in your Journal of October 1823, and which plan I now acknowledge to be my own, I have assiduously followed up that mode of arrangement; and I continue to find that it actually brings together the most closely allied productions of nature, not only more easily, but more naturally and more certainly than any other I have yet observed—I mean at least as far as I have tried it; although its various groups fall far less frequently into fives than I had at first expected.

In support of these remarks I send you hereunder a continuation of one of the branches (the Crabs) of my last table; or, to speak more in the language of modern writers, the *Brachyurous Crustacea* of the most recent authors on this intricate branch of natural science.

This present table extends no further than to the published genera (on this subject) of Lamarck, Latreille, Leach, and Desmarest; but it is quite capable of extension to the remotest species or most trivial variety that can ever occur.

Hoping that this little communication may find admission into your next Magazine,

I remain, gentlemen,

Yours, &c.

Queen's Elm, Chelsea, Nov. 19, 1824. A. H. Haworth.

P.S. The *generic names* in the table are always in *italics*, to distinguish them promptly from all others.

CRUSTACEORUM
CONSPECTUS DICHOTOMUS.

- **Brachyura.**
  - NATATORIA.
    - *LATIREMATA.*
      - Pluriremata.—*Polybius, Matuta.*
      - Biremata.
        - Rotundiora.—*Orythia, Portunus.*
        - Radiiformia (Shuttle Crabs).—*Lupa, Podopthalmus.*
    - *ANGUSTIREMATA.*—*Portumnus, Carcinus.*
  - CURSORIA.
    - *ROTUNDATA.*
      - Arcuata.
        - Conditipedes.—*Calappa, Oethra.*
        - Inconditipedes.
          - Eciliata.—*Hepatus, Cancer, Xantho, Pirimela, Pilumnus.*
          - Ciliicornes.—*Attelecyclus, Thia.*
      - Orbiculata.
        - Indomitata.
          - Sphæroidea.
            - Genuina.
              - Ecruciata.—*Leucosia, Philyra, Persephona, Myra, Ilia, Arcania, Iphis.*
              - Crucigera.—*Nursia, Ebalia.*
            - Horizontalia.—*Ixa......*
          - Turgida.—*Gecarcinus......*
        - Domitata.—*Pinnotheres......*
    - *ANGULATA.*
      - Depressa.
        - Dorsipedata.—*Dorippe, Homola, Dromia*??*
        - Thoracipedata.
          - Penicillata.—*Grapsus, Plagusia.*
          - Quadrata.—*Ocypode, Gonoplax, Uca, Eriphia, Thelphusa.*
      - Rostrata.
        - Foveata.—*Parthenope, Lambrus.*
        - Efoveata.
          - Validipedes.
            - Spuriipedes.—*Lithodes* (duæ species).
            - Communipedes.
              - Spinifrontes.—*Eurynome, Micippa, Maia, Pisa, Lissa.*
              - Emarginatifrontes.—*Hyas, Mithrax, Libinia, Docleus.*
          - Tenuipedes.
            - Fissirostres.—*Inachus, Achæus, Macropodia, Ægeria.*
            - Integra.—*Leptopodia, Pactolus.*
- **Macrura,** (*to be continued.*)

* "Genus dabit characterem, nec character Genus."

XVIII. *Comparison of Mr.* IVORY'S *Table of Refractions, Philosophical Transactions* 1824, *with Dr.* BRADLEY'S *Observations published in M.* BESSEL'S *Fundamenta Astronomiæ,* pp. 53, 54. *By* J. IVORY, *Esq. F.R.S.*

[In the paper on the Least Squares, the words "relatively to every error, and an absolute maximum relatively to all the errors" occur in the beginning of No. 2; for which read "relatively to every variable in the errors, and an absolute maximum relatively to all the variables." And the same words must be corrected in the same manner when they occur immediately below in speaking of the minima of ψ.]

THE annexed table shows the errors of my refractions when compared with the observations of Dr. Bradley cited above. In the six last stars the mean of the observations recorded by M. Bessel is taken. In the calculations the temperature of the air is estimated by the exterior thermometer, and the barometer is reduced to the standard by means of the interior thermometer. When the interior thermometer is not set down in M. Bessel's table, it is taken 5° higher than the exterior one.

It appears from this comparison that the errors of my table are very small when the zenith-distance does not exceed 88°½: but when the altitude is less than 1°½, the errors increase suddenly, and are all in defect. The same conclusion is confirmed by other computations which I have seen.

It must be observed, that in the construction of my table no assumption has been made for the horizontal refraction. The theory requires us to know only the quantity that measures the refractive power of air, and the gradation of heat in the atmosphere. The horizontal refraction obtained in this manner from theory alone is 26″ greater than what the French astronomers make it, and much less than the magnitude which the observations of M. Bessel seem to indicate. As my refractions agree with nature to so great an extent from the zenith, it seems reasonable to infer that the mean condition of the atmosphere, beyond the influence of the earth and while the aërial fluid is at liberty to arrange itself *in equilibrio* by its own inherent powers, is represented with tolerable accuracy. But in the neighbourhood of the horizon the extraneous heat and moisture emanating from the earth must derange the regular disposition which the strata of air would otherwise assume; and to this cause, for which no allowance is made in the theory, we must ascribe the sudden deviation of the theoretical refractions from those observed.

I have no other intention in publishing this comparison,

 than

than to be able to refer to it afterwards in treating of the constitution of the atmosphere.

February 8, 1825. JAMES IVORY.

Obs.	Stars.	Observed Zenith-distance.	Observed Refraction.	Barom.	Therm. In.	Therm. Out.	Error of Tabs. Ivory.	Error of Tabs. Bessel.
3	β Cassiopeæ	70° 41′ 41″·1	2′ 51″·53	30·043	40½°	35°·17	− 0″·59	− 0″·8
15	β Ursæ Maj.	70 46 30	2 44·83	29·686	46	44·17	+ 1·51	+ 1·2
8	ζ Ursæ Maj.	72 14 48·6	3 2·93	29·769	41	38·63	+ 0·68	0·0
6	α Cassiopeæ	73 17 41·4	3 14·6	29·798	44	39·37	+ 0·87	+ 0·5
10	θ Cassiopeæ	74 38 21·6	3 31·71	29·726	45	40·85	+ 0·08	+ 0·3
15	κ Cygni	75 32 12	3 49·26	29·83	38	34·05	+ 0·13	− 0·2
12	γ Persei	75 57 1·2	3 42·99	29·959	58	55·65	+ 3·23	+ 2·7
15	β Draconis	75 57 54	3 51·79	29·664	40	36·82	+ 1·9	+ 1·5
10	τ Persei	76 43 57·6	3 56·2	29·948	58	55·25	+ 3·56	+ 2·9
17	γ Draconis	76 55 31·2	4 8·98	29·686	40	37·1	+ 1·86	+ 1·5
15	ν Cygni	77 14 17·4	4 19·94	29·77	38	34·37	− 0·63	− 1·0
3	η Ursæ Maj.	77 53 18·5	4 32·87	29·93	40	35·25	+ 1·09	+ 0·7
14	θ Cygni	78 46 52·2	4 55·35	29·818	38	33·68	+ 0·1	− 0·5
15	α Persei	79 29 13·8	4 56·84	29·889	58	55·35	+ 3·6	+ 2·7
6	ν Ursæ Maj.	79 26 36	5 6·55	29·688	46	40·88	+ 0·19	− 0·3
2	2 π Cygni	80 14 57·6	5 35·4	29·68	41	34·0	+ 0·57	+ 0·2
6	θ Persei	80 16 16·2	5 28·7	30·013	52	48·42	+ 1·21	+ 0·4
12	δ Persei	81 27 18·6	6 1·9	29·924	61	58·19	+ 1·55	+ 0·8
5	ι Herculis	82 15 26·4	6 49·62	29·598	43	41·25	− 0·12	− 1·3
5	——	82 15 30·6	7 1·92	29·752	36	30·5	+ 0·31	− 0·9
22	α Aurigæ	82 41 46·8	6 52·26	29·875	67	64·68	+ 0·35	− 0·6
20	——	82 41 37·8	6 59·99	29·788	59	55·73	− 0·19	− 1·3
7	——	82 41 3	7 22·04	29·974	40	33·84	+ 1·21	+ 1·0
14	δ Cygni	83 50 33·3	8 32·97	29·805	41	37·07	− 2·15	− 3·5
11	α Cygni	83 58 16·2	8 33·79	29·79	43	40·38	+ 1·70	+ 0·7
14	——	83 58 6·7	8 48·52	29·81	36	29·95	+ 0·53	− 0·7
10	ξ Cygni	85 23 18·7	10 51·39	29·79	41	37·03	− 1·68	+ 2·8
3	μ Ursæ Maj.	85 36 50·4	10 52·90	29·887	51	50·33	+ 3·19	+ 2·2
3	——	85 36 9·3	11 4·30	29·42	43	37·67	+ 0·7	− 0·8
7	γ Andromed.	87 8 54·2	14 54·63	30·03	53	49·7	+ 1·85	+ 2·8
9	η Aurigæ	87 23 44	15 32·8	30·0	60	56·08	− 1·15	+ 2·7
6	ζ Aurigæ	87 34 7	16 15·9	30·01	58	54·91	− 2·47	+ 4·0
3	β Persei	88 12 50·5	19 49·2	29·84	46	41·33	− 2·9	+11·0
10	γ Cygni	88 39 32	23 7·94	29·80	38	34·4	−15·7	+10·7
7	α Lyræ	89 26 51·4	30 16·6	29·907	39	33·46	−39·7	+37

XIX. *Analysis of the Water of the Rio Vinagre, in the Andes of Popayan, by M.* MARIANO DE RIVERO; *with geognostic and physical Illustrations of some Phænomena which are exhibited by Sulphur, Sulphuretted Hydrogen, and Water, in Volcanos. By M.* A. DE HUMBOLDT.

[*Extract of a Letter dated the 8th October* 1823.]

"IN compliance with the desire of M. de Humboldt, I procured some of the water of Rio Vinagre. It was sent to me by M. Torrès, who takes an interest in all that can contribute to scientific researches. This water has yielded per litre:

litre: sulphuric acid, 1·080; muriatic acid, 0·184; alumine, 0.240; lime, 0.160; and some indications of iron*. The presence of muriatic acid confirms the observations made on the vapours and the stony productions of Vesuvius and of several other volcanos." RIVERO.

I had made known, at the time of my return from America, the presence of the sulphuric and muriatic acids in the water of the Rio Vinagre, which the aborigines call Pusambio (See Views of the Cordilleras, and Monuments of the People of America, vol. ii. p. 166; Barometric Levelling of the Andes, No. 126; *Caldas, Samanario del Nuevo Reyno de Granada,* t. i. p. 265); but not being furnished with the salts of barytes, I had engaged MM. Rivero and Boussingault, when they departed for Bogotá, to verify these facts. The analysis, which we owe to one of these expert chemists, is the first which has been attempted on the water of the Rio Vinagre. I shall give some extracts from the journal of my travels, in great part still unpublished, explanatory of the local circum-stances.

The town of Popayan is situated in the beautiful valley of the Rio Cauca, on the Bogota road to Quito, at the foot of the two great volcanos of Puracé and Sotarà. These volcanos, almost extinct, and exhibiting only the phænomena of solfataras, form part of the central chain of the Andes of New Granada. At 1° 55′ and 2° 20′ of north latitude, the group of mountains which incloses the sources of the Magdalena is divided into three branches, of which the eastern is continued towards Timana and the Nevados of Chita and of Merida; the intermediate and central one towards the Paramos of Guanacas and of Quimdiù; the western towards the platiniferous district of the Choco and the isthmus of Panama. In ascending from the town of Popayan to the summit of the volcano of Puracé, M. Bompland and I found at the height of 8672 feet a little plain (Llanó del Corazon), inhabited by poor Indian husbandmen. This plain is separated from the rest of the acclivity, with which it would otherwise be continuous, by two ravines extremely deep: it is at the edge of these precipices that the village of Puracé is built. Springs rise every where from the trachytic rock; each garden is surrounded

* It cannot be doubted that the indications are by grammes and fractions of grammes: a litre of the water of the Rio Vinagre includes 1·080 gramme of suphuric acid and 0·184 gramme of muriatic acid. This proportion of sulphuric acid, is nevertheless very sensible to the taste, and is manifested by an abundant precipitate with the salts of barytes.

[The litre being 2·113 pints, the contents of the water in English grains will be as follows: sulphuric acid 16·68; muriatic acid 2·84; alumine 3·7; lime 2·47.—EDIT.]

rounded with a quickset hedge of narrow-leaved euphorbium (*lechero*) of the most delicate green. This beautiful verdure contrasts in a striking manner with the back-ground of black and arid mountains which surround the volcano, and which are rent by the effects of the earthquakes.

The site of the village is celebrated in the country on account of three beautiful cascades (*choreras*) of the river of Pusambio, whose water is acid, and which the people, who know no other acid than vinegar, call *Rio Vinagre*, sometimes *Gran Vinagre*. This river takes its rise at the height of nearly 10,871 feet, in a very inaccessible spot. Although the temperature of the water be little different in the lower cascades from that of the surrounding atmosphere, it is not less certain that the sources of the Rio Pusambio or Vinagre are very hot. This fact was attested to me by the natives and by the missionary of the village of Puracé. In going to the summit of the volcano I saw a column of smoke rise at the place where the acid waters make their appearance. I have drawn the second of the falls of the Vinagre (plate xxx. of the Views of the Cordilleras): the water, which opens itself a passage across a cavern, is precipitated more than 383 feet in depth. The fall has a very picturesque effect; but the inhabitants of Popayan would be better pleased if the river, instead of throwing itself into the Rio Cauca, became engulfed in some other crevice; for such is the delicacy of constitution of animals which breathe by gills, and which absorb the oxygen dissolved in the water, that the Cauca during a course of four leagues is destitute of fish, on account of the mixture of its waters with those of the Rio Vinagre*, which are charged both with oxide of iron and with sulphuric and muriatic acid. After staying a considerable time on the craggy wall of rock which borders the cascade, a pricking sensation is felt in the eyes from the minute spray in the atmosphere. Fish re-appear in the Rio Cauca at the point where it becomes enlarged by the influx of the Pindamon and of the Palacé†.

A little to the north of the sources of the Pusambio rise two other rivulets charged in like manner with free sulphuric acid, which the people call the Little Vinegars (*los dos Vinagres chicos*): they throw themselves into the Rio de San Francisco, which is itself but a tributary of the Gran Vinagre. During my stay at Popayan, it was an opinion generally received that all these acid waters contained some iron dissolved by a

* M. Caldas has even attributed to this mixture, doubtless with little reason, the absence of goitres in the valley of Rio Cauca.—*Semanario*, t. i. p. 265. See my Memoir on the Goitres in the Cordilleras.—(*Magendie, Jour. de Physiol.* t. iv. p. 109.) † *Journal de Physique*, t. lxii. p. 61.

great

great quantity of carbonic acid. When it was merely remembered that the sources of the Vinagre are very hot, this opinion ought to have been abandoned. I boiled some water taken from the cascade; and I found, after the ebullition, the same acid taste and the same precipitates as in the unboiled water. At this period I had very few re-agents left.

The nitrate of silver* gave a white and milky precipitate, indicating the presence of muriates. The presence of iron was shown by the prussiate of lime, that of lime by the oxalate of potash. When the water was weighed with great care in the office of the mint of Popayan, the weight of an equal quantity of the water of the Vinagre was found to be to that of distilled water as 2735½ gr. to 2731 gr.; that is to say, that the specific gravity of the water of the cascade was 1,0015.

The waters which I describe, and of which M. Rivero has given the first analysis, must not be confounded with those of the two *subterraneous lakes* which we have found near the summit of the volcano; one is 14,356 feet high, the other, above the snows, 15,475 feet. This volcano of Puracé is a dome of semivitreous trachyte, of a blueish grey and having a conchoidal fracture. It does not present a great crater at its summit, but several little mouths. It differs very much from the neighbouring volcano, the Sotarà, which is of a conical form, and which has thrown out an immense quantity of obsidians. These masses, covering the plains of Julumito, are balls or *tears* of obsidian, the surface of which is often tubercular. They present, what I have seen nowhere else in the two hemispheres, all the shades of colour, from deep black to that of an artificial glass entirely colourless. It may appear surprising to see that this deprivation of colour has not been accompanied by any inflation or porosity The obsidians of Sotarà are mixed with fragments of enamel which resemble the porcelain of Réaumur, and adhering to which I have found masses of felspar which have resisted fusion.

Here, as in the Andes of Quito, as at Mexico and at the Canary Islands, the system of basaltic rocks lies far from the trachytes which form the volcanos of Puracé and of Soltarà. The basalts of the Tetilla of Julumito belong only to the left bank of the Cauca. They rise from transition porphyries free from augite, containing some hornblende, a very little quartz in small crystals embedded in the mass, and a felspar which passes from the common to the vitreous variety. This porphyry

* The conjoint presence of the sulphuric and muriatic acids has also been observed by M. Vauquelin in the water which M. Leschenault had taken from the crater-lake of Mount Idienne in Java (*Journal de Physique*, t. lxv. p. 406.) See Phil. Mag. vol. xlii. pp. 126, 182.

phyry is covered, near to Los Serillos, with a blackish-gray lime-stone traversed by veins of carbonate of lime, and so much overcharged with carbon that in some parts it stains the fingers like an aluminous schist, or like the lydian * stone of Steeben in the Fichtelgebirge. The trachytic dome of Puracé which gives birth to the little river of sulphuric acid, rises out of a porphyritic syenite (with common felspar), which in its turn is superposed on transition granite abounding in mica. This observation †, very important for the position of volcanic rocks, may be made near to Santa Barbara in ascending from Popayan to the village of Puracé. The volcano, like the most part of the great volcanos of the Andes, presents layers or mantles of melted stony matter, not real currents of lava. Some fragments of granular limestone, probably magnesian, which I found at more than 12,790 feet high, seem to have been thrown up through crevices which have since become closed. They are like those of the *Fosso Grande* of Vesuvius, which owe their granular texture to volcanic fire. It is not possible to go on horseback further than the cascades of the Rio Vinagre. From thence we were eight hours in mounting on foot to the summit of the volcano and in descending from it. The weather was dreadful; snow and hail fell. I had a great deal of difficulty in lighting the tinder at the point of the conductor of Volta's electrometer; the balls of elder-pith separated from 5 to 6 lines, and the electricity passed often from positive to negative without there being any other symptom of storm: for thunder and lightning are (according to my experience) generally very rare when we are above 12,800 or 14,000 feet high. The hail was white ‡; the hailstones, from five to seven lines in diameter, composed of layers varying in translucency. They were not only much flattened towards the poles, but so much increased in their equatorial diameter, that rings of ice separated themselves on the least shock. I had

* M. Vauquelin has recently proved by a direct analysis the presence of carbon in the purest lydian stones. I had found, in a series of experiments made on the galvanic exciters in 1798, that the lydian stones of the transition schists of Steeben produced jointly with zinc the same effect as graphite or carburet of iron. I have since made some trials to prove chemically the presence of carbon in several varieties of lydian stone.—See my Experiments on the Nervous and Muscular Fibre (in German), t. ii. pp. 163.

† See an account of the whole of these phænomena of the volcanos of Popayan in my *Essai sur le Gisement des Roches*, 1823, pp. 129, 139, 340.

‡ I have already remarked elsewhere in the *Ann. de Chimie*, that at Paramo de Guanacas, where the road from Bogóta to Popayan passes to the height of 14,700 feet, there has been seen fall, not snow, but red hail. Did it inclose those same germs of vegetable organization which have been discovered above the polar circle?

had already twice observed and described this phænomenon, in the mountains of Bareuth, and near Cracow, during a journey in Poland. Can it be admitted that the successive layers which are added to the central nucleus are in a state of fluidity so great that the rotary motion can cause the flattening of the spheroids?

When the barometer indicated that we were come very near the limit of perpetual snow, we found the masses of sulphur disseminated in imperfectly columnar trachytic rocks augmented. This phænomenon struck me the more, as I knew how rare sulphur is on the sides of inflamed volcanos:—a column of yellowish smoke and a frightful noise informed us of the neighbourhood of one of the mouths (*bocas*) of the volcano. We had some trouble to approach its edge; the declivity of the mountain being very steep, and the crevices only covered by a crust of sulphur, of whose thickness we were ignorant. We believed we might rate the extent of this crust, which is often interrupted by rocks, at more than 12,000 square feet. These little ridges of trachytic rocks act strongly on the magnet. I tried to keep at as much distance from them as possible, to determine the inclination of the needle. It was at the town of Popayan (height 5825 English feet) 23°,05, centesimal division; at the village of Puracé (height 8671 feet) 21°,81; near the summit of the volcano of Puracé (height 14,542 feet) 20°,85. The intensity of the magnetic force varied very little at Popayan and at the village of Puracé; and the diminution of the inclination is certainly not the effect of the height, as is proved by so many other observations which I have made on the summit of the Andes, but the effect of local attractions depending on certain centres of action in the trachytes.

The mouth of the volcano of Puracé is a perpendicular cleft, the visible opening of which is only 6 feet long and 3 broad. It is covered in form of a vault by a layer of very pure sulphur, which is 18 inches thick, and which the force of the elastic vapours has split on the north side. At the distance of 12 feet from the mouth we felt an agreeable heat. The centigrade thermometer, which had kept till then at 6°,2 (43° F.) (a cold not at all considerable in a time of hail, and at a height of 14,356 feet), rose to 15° (61° F.). Placed in such a manner as not to be incommoded by the vapours, we had the pleasure of drying our clothes. The frightful noise which is heard near this opening has almost always the same intensity: it can only be compared to that which would be caused by several steam-engines together, were the dense steam suffered to escape from all at the same moment. We threw great stones into the crevice, and we discovered on this occasion that the

opening communicated with a basin full of boiling water. The vapours which escape with so much violence are of the sulphurous acid, which is indicated by their suffocating smell. We shall soon see that the water of the subterraneous lake is charged with sulphuretted hydrogen; but the odour of this gas is not smelt at the summit of the volcano, because it is disguised by the much stronger smell of the sulphurous acid vapours. I had not any means of determining the temperature of these vapours, which seem to undergo a prodigiously strong pressure in the interior of the volcano. As the Indians pretend that the opening has several compartments which are not all filled with water, and that the noise which is heard at times in the interior of the crevice is the forerunner of flames, I introduced, by means of a long pole, some papers coloured with the tincture of violet, under the vault, where I could be sure of not touching the surface of the water. Drawing back the pole, I found the papers strongly reddened, but not at all inflamed, as was easy to be foreseen.

We succeeded after several vain attempts in obtaining some water from the crevice: this was by tying a *tutuma* (the fruit of the *Crescentia Cujete*) to a stick 8 feet long. The water was directly poured into a bottle hermetically stopped. We examined it on our return to the village of Puracé: it exhaled a strong smell of sulphuretted hydrogen; it had no acid taste, but some weak precipitates caused by the nitrate of silver showed the presence of muriatic acid. The crust of sulphur which forms above the mouth arises without doubt from the contact of the vapours of sulphurous acid with the sulphuretted hydrogen which the subterraneous lake disengages. Even the water of this lake is covered with a coat of sulphur, which disappeared in the places where we threw the stones. It results from these observations, that only the presence of the muriatic acid, or of combinations of this acid with salifiable bases, indicates a feeble analogy between the waters of Rio Vinagre and those of the lakes. The first, which spring much lower, at the declivity of the volcano of Puracé, are charged with free sulphuric acid: the others, which are found at the summit of the volcano, contain sulphuretted hydrogen. As the upper mouths are found at very different heights above the level of the sea, it may be supposed that their subterraneous waters are owing to the melting of the snows, and that they do not communicate. The Rio Vinagre receives its acid in the interior of a volcano which abounds in sulphur, and the temperature of which appears extremely elevated, although for centuries no luminous phænomenon has been perceived at its summit.

The

The good curate of the village of Puracé thought to render a great service to his parishioners, as well as to the inhabitants of the town of Popayan, in causing, as he said, the *chimneys of the volcano* to be cleaned now and then. He ordered the Indians to take away the crust of sulphur which rises in form of a dome above the crevice. This crust has acquired sometimes, as they affirm, a thickness of as much as four feet in less than two years. It lessens without doubt the opening by which the vapours of sulphurous acid escape; but it may be conceived that the elastic force of these vapours is such that, if the opening were entirely stopped up for some moments, it would sooner break the new arch than produce commotions by acting against the rocky sides of the volcano. For several years the lakes, which represent in miniature the *crater-lakes* of our extinguished volcanos, seem each to preserve the same level of their line of water; which proves that the evaporation is equal to the infiltration of the waters of snow and rain: but this equilibrium has not always been equally steady. About the year 1790 the *Boca grande* caused partial inundations. I dwell on this phænomenon, because it seems to throw some light on a problem of the geology of volcanos, which has not been sufficiently examined: I mean the ejections of water and mud. At Vesuvius these ejections are only apparent, and come neither from the interior of the crater nor from the lateral crevices. An immense electric tension manifests itself in the atmosphere which surrounds the summit of the volcano at the time of great eruptions. Flashes of lightning cleave the air; the aqueous vapours thrown out by the crater are cooled; thick clouds envelop the summit during the continuance of this storm, confined to a little space; the water descends in torrents, and is mixed with the tufaceous substances which it drags with it*. These effects, purely meteorological, have given rise to the traditions about boiling waters that issued from the crater of Vesuvius

* M. de la Condamine (*Mémoires de l'Académie* 1754, p. 18) had already expressed very precise ideas on the cause of these phænomena, which are found equally well explained in the *Storia dell' incendio del* 1737, published by the Academy of Naples. I saw in my last journey to Naples, (December 1822,) the ravages caused by the torrents of water from the side of Ottajano, at the foot of Vesuvius. They had transported into the plain, not only mud, but masses of lava 48 feet in circumference and 25 feet high. See the excellent description of these phænomena by MM. Monticelli and Covelli. (*Storia del Vesuvio degli anni* 1821—1823, p. 91—98.) Phil. Mag. vol. lxiii. p. 46. By the mixture of the rain and the volcanic cinders, there is formed in the air (*l. c.* p. 94) a kind of pisolites with concentric layers, which I also found on the plain of Hambato, among the ancient ejections of the Carguairazo. The inhabitants of the province of Quito call these pisolites *earth hailstones.*

in 1631; fabulous traditions, which are perpetuated by an inscription at Portici.

In the volcanos of the Andes which exceed the limit of perpetual snow, the causes of inundations are very different from those which we have just indicated. As the eruptions of these colossal summits take place only after long intervals (every thirty or forty years, or still more rarely), banks of snow of an enormous thickness accumulate on the sides of the mountains. These snows do not melt at the time of the explosion only, but sometimes several days before. Thus in February 1803, during my stay at Guayaquil, the inhabitants of the province of Quito were frightened at the appearance of the cone of Cotopaxi, which lost a great part of its snows in a single night, and showed plainly the black colour of its burnt rocks. Whatever idea may be formed of the power of the volcanic forces, and of the intensity of the subterraneous fires in the Andes, it cannot be admitted that the thick sides of a cone could be uniformly warmed, and transmit the heat with such rapidity (by the conductibility of their mass) to the outside. The sudden melting of the snows, when, in the Cordilleras, it precedes the eruptions, is probably owing only to an infinity of little *fumaroles* which disengage hot vapours through the fissured rock of the cone. These vapours, according to what I have had opportunity of observing in the craters of Vesuvius, the Peak of Teneriffe, and the volcano of Jorullo in Mexico, are most frequently pure water, which does not act at all on the most sensible re-agents; at other times they contain muriatic acid. It is remarked that the same crevice gives, at very near epochs, distilled (pure) water and very acid waters. The artificial spring which M. Gimbernat has had the ingenious idea of forming at the summit of Vesuvius, by the condensation of the vapours in a glass tube, has sometimes shown these variations: they prove either the change of chemical action in the interior of the volcano, or the accidental opening of some new communications. In the Andes of Quito, as in Iceland, and in the eruptions of Ætna of March 23, 1536, and March 6, 1755, the sudden melting of the banks of snow produced great devastations*.

At other times, by slow infiltrations the snow waters are accumulated in the lateral cavities of the volcano; shocks of violent earthquakes, which do not always coincide with the epoch of the fiery eruptions, open these cavities; and waters long kept in, which support little fish of the genus *Pimelodes*, carry with them pulverized trachytes, pumice-stones, tufas,

* Ferrara, *Campi Flegrei*, 1810, p. 165.—Idem, *Descriz. dell' Etna*, 1818, p. 89, 116—120.

and

and other incoherent matters. These liquid ejections spread sterility over the plains for centuries. Muddy clays (*lodazales*) covered a space of more than four square leagues, when, in the night of the 19th of June 1698, the Peak of Carguairazo, the actual height of which exceeds 15,700 feet, sunk down with a noise. The lakes of sulphureous water that we found at the summit of Puracé, explain what the inhabitants of Quito report of the fetid smell of the waters which descend sometimes from the sides of the volcanos during great eruptions. Struck with the novelty of these phænomena, which we only mention here, the Spanish *Conquistadores* have, since the sixteenth century, distinguished two sorts of volcanoes,—the *fire volcanos* and the *water volcanos* (*volcanes de fuego y de agua*). This last denomination, which one might say was invented to bring near to each other the *volcanists* and the *neptunists*, and to put an end to the famous schism of dogmatical geology, has been applied especially to the mountains of Guatimala and of the Archipelago of the Philippines. The *Volcan de agua*, placed between the volcano of Guatimala* and that of Pocaya, ruined, by torrents of water and stones which it sent forth the 11th of September 1541, the town of Almolonga, which is the ancient capital of the country. This mountain does not attain the limit of perpetual snow, but it remains covered with snow several months of the year. When we call to mind the confusion of the accounts that are found in our own days in the public papers of Europe, every time that Ætna or Vesuvius are in action, we cannot complain of the uncertainty in which the chroniclers of Spanish America and the *Conquistadores* of the sixteenth century leave us respecting the phænomena of *volcanic inundations*, so worthy of engaging the attention of natural philosophers. During the eruption of Ætna in 1792, there opened on the declivity of the volcano, 3 miles from the crater, a gulf† from which issued for several weeks water mixed with ashes, scoriæ, and clays. These liquid ejections, which must not be confounded with the phæ-

* Juarros, *Compendio de la Historia de Guatemala*, 1809, t. i. p. 72; t. ii. p. 351.—Remesal, *Hist. de la Provincia de San-Vicente*, lib. iv. cap. 6.—Also in the great eruption of the volcano of the province of Sinano in Japan (July 27, 1783), boiling waters were mixed with the *rapilli*. (*Mémoire sur la Dynastie régnante des Djogouns.* 1820, p. 182.)

† *Ferrara, Descr. dell' Etna*, p. 132. As this phænomenon seems to have some relation to that of the *Moya de Pelileo*, which contains the carburets of hydrogen, and which I made known at my return from America, I obtained very lately an explanatory manuscript note from the learned Sicilian geologist, M. Ferrara, on the muddy eruption of Ætna observed March 25, 1792.

nomenon

nomenon of the *Salses**, or *air volcanos*, were very thick. It is easily conceivable that, in the equinoctial zone, even very low mountains may by a particular disposition of their subterraneous cavities, and by the excessive abundance of the tropical rains, be subject to cause frightful inundations each time that they undergo shocks of earthquakes. Furthermore, the phænomena which we have been describing are repeated from time to time far from the volcanos, in secondary mountains, in the centre of Europe. Sad examples have proved in our days that in the Alps of Switzerland, where no shocks of earthquakes are felt, a simple hydrostatic pressure lifts up and breaks with violence banks of rock, throwing them to a great distance, as if they were projected by elastic forces.

The trachytes of Puracé contain sulphur like those of Mont-Dore in Auvergne, of Budoshegy in Transylvania, of the Isle of Montserrat in the Little Antilles, and of the Antisana in the Andes of Quito. It is still formed daily in the clefts around the gulfs of Puracé, either by a very slow sublimation, or by the contact of the sulphurous acid vapours with the sulphuretted hydrogen of the lake. The volcano labours in its interior like the *solfataras;* but it presents nothing in its form that resembles the places which are designated by that name, and which I have visited; for example, the *solfataras* of Puzzuoli, the Peak of Teneriffe, and the volcano of Jorullo in Mexico. These last three are craters which have vomited lava; they show that their first state was very different to that in which we see them at present. With very elevated temperatures, the chemical products of a volcano are not the same as with a very low temperature. If the appellation *solfatara* be given indefinitely to every place where sulphur is formed or deposited, this denomination may also be applied to a district which I shall describe here, and which contrasts singularly with the trachytes of volcanos. In crossing the Cordilleras of the Andes of Quindiu, between the basins of the Cauca and of the Magdalena (lat. 4° 50′—4° 45′) I saw an immense formation of gneiss and of micaceous schist resting immediately on an ancient granite. The layers of micaceous schist which alternate with strata of gneiss are free from garnets, whilst the gneiss contains many. But, in these same primitive micaceous schists, a little to the west of the station of the Moral, at the height of 6800 feet above the level of the sea, in the *Quebrada del Azufral*, some *decayed veins*

* There is only the muddy torrent (*fiume di fango*) of Santa-Maria-Nascemi (March 18, 1790), in the Val di Noto, which seems to me to belong to the action of the Salses.

extremely

extremely full of crevices abound in sulphur*, and exhale a sulphureous vapour, the temperature of which rose to 47°8 centesimal (118° F.), when the surrounding air was at 20°2 (68°F.). Here then is repeated on a small scale, in the clefts of a primitive rock, the phænomena of the trachytic *solfatara* of Budoshegy in Transylvania, which has been recently examined by M. Boué. The micaceous schist of Quindiu, which surrounds the *open veins*, is decomposed, and the sulphur is formed in masses considerable enough to become the object of a sulphur-work which supports a family settled in the ravine of the Azufral. The rock contains some decomposed pyrites; but I much doubt whether these pyrites perform the important part in nature which has been so long ascribed to them in geological treatises. In the midst of the granitic rocks of Quindiu rise the trachytes of the volcano of Tolima, a truncated cone, which reminds us of the form of the Cotopaxi, and which, according to a geodesic measurement made by me at the west of Ibagué, is the highest summit of the Andes in the northern hemisphere†. A rivulet which emits considerably the smell of sulphuretted hydrogen descends from the Peak of Tolima, and proves that the trachytes which have penetrated the granitic rocks also contain sulphur. Two learned travellers, MM. Rivero and Boussingault, have recently visited this little *solfatara in the micaceous schist* of Quindiu: they have sent some specimens to the cabinet of the *Ecole des Mines* at Paris, which contains the most complete and instructive series of geognostic specimens. Following the Cordillera of the Andes southwards, these same alternations of primitive formations and of porphyritic and trachytic regions are found:—but what was my surprise, when beyond the equator I ascertained that the celebrated *mountain of sulphur of Ticsan* (S. lat. 2° 10′), between Quito and Cuenca, is neither composed of trachyte, nor of chalk or of gypsum, but of micaceous schist.

This mountain of sulphur, which the Indians call *Quello*, is situated, according to my barometric measurement, at the height of 8000 feet above the level of the ocean. It is entirely composed of primitive micaceous schist (*glimmerschiefer*), which is not even anthracitic, as are the varieties of this rock peculiar to transition countries. In some very deep ravines between Ticsan and Alausi, the micaceous schist is seen resting on gneiss. The sulphur is contained in a stratum of quartz which is more than 1200 feet thick: it lies in a tolerably regular direction N. 18° E., and inclined like the micaceous

* See my Barometric and Geognostic Levelling of the Cordilleras, No. 102.
† Height 18,321 feet; N. lat. 14° 46′.

schist

schist from 70° to 80° to the north-west. The bed of quartz, which passes sometimes into the hornstone, is wrought in an open working. The declivity of the *Cerro Quello*, on which the works were begun some centuries since, is opposite to the south-south-east; and the bed of quartz appears to be prolonged towards the north-north-west, that is to say, towards the coast of the Pacific Ocean. It is however asserted that sulphur has not been found on the surface of the ground in this direction to the distance of 2000 toises from Ticsan. All is covered there with a thick vegetation. Towards the end of the eighteenth century, masses of sulphur were still worked, which were from 2 to 3 feet in diameter. At present they are working some quartzose strata much less rich, in which the sulphur is only dispersed in nodules from 3 to 4 inches thick. It is observed that the quantity of sulphur increases with the depth; but the working has been so unskilfully directed that the lower strata are nearly inaccessible. The quartz in which the sulphur is dispersed presents neither great fissures nor cavities, or *druses;* nor have I been able to find any specimen of crystallized sulphur.

The mineral which is the object of the working of the *Cerro Quello* does not form a mass or complication of veins, as might be supposed: the sulphur is disseminated without any continuity by little masses in the quartz which traverses the micaceous schist in a direction parallel to its strata. The clefts that have perhaps formerly united these masses are no longer visible, but all the quartz seems to have undergone an extraordinary change. It is tarnished, often brittle, and breaks in some parts on the least shock; which indicates an imperceptible cleavage. The temperature of the rock did not differ from that of the exterior air. The inhabitants like to attribute the violent earthquakes to which their country has been sometimes exposed to concavities which they suppose to exist under the mountain of sulphur. If this hypothesis be well founded, it must be admitted that the cause which it indicates acts but locally. In the great catastrophe of the 4th of February 1797, which destroyed so many thousand Indians in the province of Quito,—the three places where there is the most sulphur, the Cerro Quello, the Azufral of Cuesaca near to the Villa of Ibarra, and the Machay of Saint-Simon, near the volcano of Antisana, were but very feebly agitated; but at a much earlier period there has been experienced, even on the bed of quartz which includes the sulphur near Ticsan, an explosion similar to that of a mine.

The bed of quartz appears at the surface on the two sides of the little river of Alausi; and facing the Cerro Quello is found

found a little plain, where, in the seventeenth century, was situated the village of Ticsan. The ruins of the church of *Pueblo Viejo* are still seen. An earthquake wholly local (for its effects were confined to a very small space of country) made the surrounding hills sink down: a part of the village sunk; another part was thrown into the air, as happened at Riobamba, where I found the bones of the unfortunate inhabitants of the town thrown on the Cerro de la Culca, to a height of several hundred feet. The Indians of Ticsan who survived this catastrophe constructed their habitations more to the north, far from the mountain of sulphur whose neighbourhood they dreaded. It may be that the coincidence of these phænomena of explosion and of the position (*gisement*) of a substance easy to be converted into elastic vapours has only been accidental: but it may be also that ancient communications with the interior of the globe, those upon which is formed by sublimation the immense deposit of sulphur, become re-established from time to time, and allow the volcanic forces to shake the surface of the soil. Near the ruins of *Pueblo Viejo* of Ticsan I found a hill of gypsum lying above the micaceous schist: as this hill is not covered by other formations, it is difficult to decide whether the gypsum, partly fibrous and mixed with clay, is primitive, like that of Val Canaria, or transition, like the gypsum of the Tarentaise.

The abundance of sulphur in primitive countries is a very important geological fact, in relation to the study of volcanos and of rocks through which the subterraneous fire has opened itself a passage. Before I had visited the Andes of Quito and the mountain of Ticsan, sulphur was known only in the transition limestone and gypsum; in the gypsums, marles and muriatiferous clays of secondary countries, and in the rocks exclusively called *volcanic*. These different geological situations, to which may be added the tertiary districts, very ill explained the frequency of the sulphureous vapours exhaled by the mouths of the volcanos whose centre of action was placed (and doubtless with propriety) very much below the secondary and intermediate rocks. In proportion as we become acquainted with a greater part of the globe we not only see positive geognosy, that is to say, the view of the formations and of the geological positions, extended; but even *geogony*, or systematic geognosy, the conjectural science which investigates the causes of phænomena, begins to be founded on the analogy of more certain facts. We may have been struck for some time past with the little masses of native sulphur which are disseminated in some metalliferous veins, and which traverse granitic rocks; for example, in Schwarzwald, near Riepoldsau. The mountain

of Ticsan which I have made known, leaves no further doubt respecting the existence of sulphur in the primitive districts. It has also been lately found in Brazil, that the chloritic quartz formation which covers, in the Capitania de Minas Geraes, the primitive clay-slate, contains both gold and sulphur. Laminæ of this rock strongly heated burn with a blue flame. Near to Villarica, in the district called Antonio Pereira, a schist, of the same age as that on which is superposed the itacolumite or chloritic quartz, contains a calcareous bed traversed by veins of quartz, which the Baron d'Eschwege (director of the gold and diamond mines of these countries) has found filled with little nodules of pulverulent sulphur. All these phænomena increase in interest, when we reflect that this learned geologist, and also another German traveller (M. Pohl) incline to the opinion that gold, micaceous iron, diamonds, euclases, platina, and palladium, which are peculiar to the alluvial districts of Brazil, have been derived either from the destruction of the great formation of chloritic quartz, or from that of a ferruginous bed (*itabarite*) which is placed above this formation.

XX. *On the Nature and Properties of Indigo; with Directions for the Valuation of different Samples. By* John Dalton, *Esq. F.R.S. &c.**

We owe the first good approximation to the chemical analysis of the indigo of commerce to Bergman. According to his experiments, the best samples of indigo yielded, by analysis, the following principles:

47	pure indigo
12	gum
6	resin
22	earth
13	oxide of Iron
100	

A subsequent analysis of indigo made by Chevreul (*Annal. de Chimie*, t. 68), gives 45 per cent of pure indigo in the best Guatimala indigo, and the foreign matters much the same as by Bergman, but differing considerably in the proportions. Indeed it is most probable that the foreign matters will be found to differ materially, both in quantity and kind, from the various modes and circumstances of the manufacture as practised in different places, and perhaps from the various species

* From the Memoirs of the Literary and Philosophical Society of Manchester.

of

of plants from which the indigo is extracted in different parts of the world.

It is to be understood that the part called pure indigo is the sole colouring matter, and that which gives value to the article. The rest may be considered as dross, doing no good, and being probably harmless to the use of the drug as a dye, but scarcely so to the printer, who meets with obstructions enough in the exercise of his art, without introducing such as may easily be avoided.

When we consider, however, that indigo is produced by a species of fermentation from vegetable matter, analogous to the vinous and acetous fermentation of saccharine matter, it is not improbable that the fermentation in many cases may be incomplete. And as the foreign matter found in the indigo of commerce is chiefly vegetable, and composed of the same elements as pure indigo, it may by a fresh fermentation develop more of the pure indigo than is found in it originally. This conjecture is countenanced by the practice of dyers, who, when the indigo is nearly spent, as the phrase is, put in other vegetable matter to the residue, and by certain processes obtain an addition to the quantity of colouring, which otherwise would not be acquired. In a similar way I conceive it is that vinegar made from sugar often contains a considerable portion of the latter, which has escaped the fermenting process.

There are two ways of obtaining pure indigo. The one is that commonly practised by dyers in their use of the article. On a small scale it may be effected as follows: into a two-quart bottle put 50 grains of finely pounded indigo, three or four times as much sulphate of iron, and hydrate of lime same weight as the salt of iron. Then fill the bottle with water, leaving little more room than what the cork or stopper will occupy. Mix up the contents by repeated agitation, and then let the insoluble matters subside. A fine transparent greenish-yellow liquid will appear in a day or two, which must be drawn off carefully by a syphon. As soon as this liquid is agitated in the air it becomes opaque, and a precipitate is formed, which is pure indigo; but it cannot be collected without some carbonate of lime in the first instance; it must therefore be submitted to water acidulated with muriatic acid, which dissolves the lime, and leaves the pure indigo to subside. Afterwards it may be collected on a filtre and dried. The theory of this process is now well understood. Pure indigo, deprived of a certain portion of oxygen, is known to be soluble in lime-water; the protoxide of iron, precipitated by the lime, deprives it of this oxygen, and hence the solution of the de-oxidized indigo. Such, however, is the affinity of in-

digo in this state for oxygen, that it resumes it from atmospheric air the moment they are brought into contact.

Pure indigo thus obtained is called *precipitated* indigo: the solution may also be had from a blue-dyer's vat by plunging an empty phial into the liquid a few inches below the surface.

The other way of obtaining pure indigo is by sublimation. Take 20 or 30 grains of pulverized common indigo and place it in an iron spoon, which must be gradually heated to 500° or 600° Fahrenheit. A purple smoke will then exhale copiously, and at the same time a fine tissue of small, shining, silky needles will start up on the surface of the indigo. These may be withdrawn by the point of a knife; they are crystals of sublimed indigo.

Precipitated and *sublimed* indigo appear by the chemical tests to be constituted of the same elements; and no doubt is entertained that they present the pure colouring matter of indigo in its most concentrated form.

Three chemists have published analyses of pure indigo within the last three years; namely, Drs. Thomson and Ure, and Mr. W. Crum, all of Glasgow. The same plan was adopted by all three; namely, burning a small given portion of indigo in contact with the black oxide of copper in green glass tubes. The indigo being finely divided and intimately diffused through a comparatively large portion of the oxide, heat is applied sufficient to burn the carbon and hydrogen of the indigo, and to liberate the azote: hence from the quantities of carbonic acid and azote produced, and the loss of weight which the oxide sustains, the constituents of indigo are inferred. The results are below:

	Dr. Thomson.		Dr. Ure.		Mr. Crum.
Carbon	40·39	—	71·37	—	73·22
Azote	13·46	—	10.	—	11·26
Oxygen	46·15	—	14·25	—	12·60
Hydrogen	0	—	4·38	—	2·92
	100		100		100

It is observable that the results of Dr. Ure and Mr. Crum present no remarkable differences, except in regard to hydrogen; whilst Dr. Thomson finds no hydrogen: and remarkable differences between his results and those of the other two are found in the articles carbon and oxygen.

The atomic constitution of indigo by the above authors is as follows:

	Dr. Thomson.		Dr. Ure.		Mr. Crum.
Carbon	7 atoms	—	16 atoms	—	16 atoms
Oxygen	6 do.	—	2 do.	—	2 do.
Azote	1 do.	—	1 do.	—	1 do.
Hydrogen	0 do.	—	6 do.	—	4 do.
	14		25		23

I am

I am inclined to think the analysis of Mr. Crum as likely to be an approximation to the constitution of pure indigo as either of the other two; and I should adopt his atomic constitution, if he would modify it so as to adopt my weight of the atom of azote instead of its double, which has somehow got into common reception as a substitute without any sufficient reason that I can find. If we adopt my weight for azote, Mr. Crum's atoms will become 16, 2, 2, and 4; which, being all divisible by 2, become

8 atoms carbon
1 atom oxygen
1 atom azote
2 atoms hydrogen

12

This simplification of the atom of indigo I suggested to Mr. Crum in a conversation we had together, and he seemed inclined to adopt it. Referring therefore to my scale of atomic numbers, we shall have the atom of pure indigo to consist of

8 atoms carbon	5·4	=	43·2	......	75·5
1 atom oxygen	7	=	7	......	12·3
1 atom azote	5	=	5	......	8·7
2 atoms hydrogen	1	=	2	......	3·5
			57·2		100

Mr. Crum, in his very ingenious essay above referred to, finds that a compound of one atom of indigo and one of water may be formed by means of sulphuric acid: he denominates it *phenicin;* it may perhaps be better designated by the name of proto-hydrate of indigo. The common product of sulphuric acid and indigo, or *sulphate of indigo* of Dr. Bancroft, he calls *cerulin*, and finds it to be a compound of one indigo and two of water, or the deuto-hydrate of indigo.

I have made no attempts myself to analyse pure indigo into its constituent elements, but have often tried, both recently and some years ago, to find the quantity of oxygen required to convert the green indigo solutions in lime-water into blue indigo. The results have been pretty uniformly the same—namely, that the oxygen which combined with the green indigo, to convert it to blue, was about one-seventh or one-eighth of the whole weight of the resulting indigo; and hence I concluded, on the supposition that one atom of oxygen was added to one of indigo, that the atom of indigo must weigh about 55, or 56; and this conclusion I pointed out to Mr. Crum, as corroborating his analysis. The quantity of oxygen required was much less, and of course the weight of the atom of indigo was much greater than I had anticipated.

We

We now proceed to the consideration of the best means of fixing a comparative value upon the different samples of the indigo of commerce. After numerous trials I find the method first suggested by Descroisille to judge of the strength of oxymuriatic acid to be preferred. The objects indeed are different, but the operations are analogous: he made use of a given solution of indigo to ascertain the comparative strengths of various solutions containing oxymuriatic acid; on the other hand, I propose to use a solution of oxymuriatic acid of known strength, to compare the relative quantities of pure indigo in different samples.

In the first volume of the Annals of Philosophy (1813) I pointed out a safe and easy method of estimating the quantity of oxymuriatic acid, in solutions of oxymuriate of lime, not by solutions of indigo, which must be variable from the quality of the indigo, but by solutions of proto-sulphate of iron, which can always be obtained of the same strength. I say *safe* and *easy* method, notwithstanding we are gravely told by one professor of chemistry that he had tried the method and was nearly killed by it; and another states that he had attempted to follow my method, but did not succeed. Any person who is tolerably skilled in chemical manipulations, and has the two liquids, sulphate of iron of a known strength, and oxymuriate of lime of a known specific gravity, before him, may determine the strength of the oxymuriate in the space of five minutes. In this time I found the strength of the oxymuriate of lime used on the present occasion. Having by me a solution of proto-sulphate of iron containing eight per cent oxide, I took 50 grain measures of it and poured them into a wine glass, then 100 of the oxymuriate, stirring the mixture, after which no smell was perceived; 100 more were poured in, still no smell; then dropped in ten grains at a time by a dropping tube, stirring the mixture each time; the fifth 10 grains produced a slight but transient smell; the sixth a strong and permanent smell. Hence 250 were required to saturate 50 of the sulphate. The oxide (four grains) being divided by nine, gives ·444 for the weight of oxygen in 250 oxymuriate, or ·17 parts of a grain of oxygen are imparted by each hundred of the liquid.

In the essay above referred to I mentioned another method of investigating the value of oxymuriate of lime solution; but, owing to the then prevailing erroneous notion on the proportion of the elements of nitric acid, no satisfactory application could be made of it. I now find that oxymuriate of lime converts nitrous gas into nitric acid immediately; and hence this operation may be used with great elegance and

precision

precision to show the real quantity of oxymuriatic acid in solutions.

For example: I took a graduated tube of capacity equal to 300 grains of water; I filled it with pure nitrous gas, and then transferred it to a cup of the liquid oxymuriate, valued above by the sulphate of iron. After repeated agitation, covering the end of the tube carefully with my finger, I soon had 100 measures of liquid in the tube: then withdrawing it to a cup of water I agitated repeatedly, letting in water each time, instead of oxymuriate of lime, because I was aware that the 100 measures already in the tube were not saturated. Soon after, the process was at an end, no more nitrous gas being absorbed. The 100 measures of the oxymuriate took 168 measures of nitrous gas to saturate them. Now deducting one-sixteenth of this for the nitrous gas impregnating the liquid, and for loss occasioned by the free oxygen gas in the water which the nitrous gas had to combine with, there will remain 157 nitrous gas $= \cdot 2$ grain weight, which was converted into nitric acid; but if we deduct one-eighth part from the weight of nitrous gas, we shall have the weight of oxygen requisite to convert it into nitric acid $= \cdot 175$ parts of a grain; only differing $\frac{2}{1000}$ from the other valuation by sulphate of iron.

To find the value of any sample of indigo, I take one grain carefully weighed from a mass finely pulverized. I put this into a small glass, a wine-glass for example; then by a dropping tube I put two or three grains of concentrated sulphuric acid upon it. The two principles are next well mixed together by trituration with the end of a small glass rod. Water is then poured in, and the colouring matter fully diffused through it. The liquid is now transferred into a tall cylindrical jar, of about one inch internal diameter; more water is poured in till the mixture becomes sufficiently dilute to show the figure of the flame of a candle through it. Then the liquid oxymuriate is mixed with the liquid gradually and by measure, agitating duly each time, and never putting any more in till the smell of the preceding has vanished. The liquid soon becomes transparent and of a beautiful greenish-yellow appearance; after the dross has subsided, the clear liquid may be poured off, and a little more water put to the sediment, with a few drops of oxymuriate of lime, and a drop of dilute sulphuric acid; if more yellow liquid is produced, it arises from particles of indigo which have escaped the action of the oxymuriate before, and must be added to the rest.

The value of the indigo I consider in proportion to the quantity of real oxymuriate of lime necessary to destroy its colour. The value also may be well estimated by the quantity

and

and intensity of the amber-coloured liquid which the indigo produces, and this is found independently of any valuation of the oxymuriate of lime.

Some of the samples I have tried, and the results are as under:

1.—*Precipitated* and *sublimed* indigo, each one grain, gave nearly the same results. Each of these required 140 grains of the oxymuriate of lime solution, corresponding to 25 parts of a grain of oxygen. The yellow liquid obtained was 3600 grains.

2.—Flora indigo, one grain, required 70 of the oxymuriate =·125 parts of a grain of oxygen, or one half of the other.

The same result from a sample marked J. R. *best.*

3.—Indigoes marked 1 P and 3 P required about 60 of the oxymuriate.

4.—Those marked J. R. *middle*, J. R. *worst*, and 4 P, required about 50 oxymuriate.

5.—That marked *Wood* was rather inferior to the above, but required above 40 oxymuriate.

6.—Those marked 2 P and 1194 were the lowest I have examined; one grain of each did not require more than 30 oxymuriate, or 35 at the most. A poor turbid yellow liquor was produced. The sample 2 P, when burned, yielded about 30 per cent of fine sand.

Upon a review of these experiments, I am persuaded that to destroy indigo by oxymuriatic acid, *twice* the quantity of oxygen is necessary that is required to revive it from the lime solution.

I hope the subject here taken up will not be considered as unimportant, when we are informed that the article *indigo* imported into this country annually, about fifteen years ago, amounted in value to upwards of two millions of pounds sterling; and it much exceeds that sum in all probability at the present time.

XXI. *On Aërial Navigation.* *By* A Correspondent.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE late passion for balloons has subsided without producing any improvement, except a sensible hint in one of the newspapers, that the aëronaut should carry with him a long line having a great number of ropes attached, so as to increase the chance of assistance from below. Yet I think it will appear that the direction of these machines, though

though a matter of the greatest difficulty, is by no means impossible. The principles are well worthy the attention of the scientific mechanist, who would be most usefully employed in clearing the way, by removing false principles. If he cannot attain complete success, let him at least contribute any useful hints, let him feel superior to the ignorant ridicule thrown upon the subject, since no man of science ever touched a question, and left it as he found it.

The balloon, by being carried with the same velocity as the current of air, wants what sailors call "head-way," or "steerage-way." Its indifference of position cannot be modified by keel, rudder, or sail, unless it acquire a *relative velocity* with regard to the current, unless it moves slower or faster than the air in which it floats. If your readers have read of, or thought of, any means of producing such an effect, even remotely plausible, except the following, it would be desirable that they should appear in your Journal.

1. It has been suggested, that by ascending and descending while a plane attached to the balloon maintains given angles to the current, the resolution of forces will enable us to make some progress. The best method of bringing into action this, and indeed all other principles for the guidance of these machines, will be to unite a larger and a smaller balloon by means of a platform. This platform may be lowered or raised at any angle, by sliding a weight towards that balloon which we may wish to depress. That the principle of angular motion will be occasionally used, especially in calm weather, is highly probable, whatever other means shall succeed; but that this alone will be effectual, can scarcely be expected.

2. It has been proposed to connect the balloon with a lower stratum of the air, of greater density and less velocity, so as to retard the motion of the machine. This would allow us to employ a keel, rudder, and sails; and though we could not proceed against the current, it would enable us to deviate a few degrees to the right or left of the general direction.

3. It has been proposed to act on the air, as the tail of a fish acts on water, or as a single oar behind acts in the motion of a boat. The principle is not generally understood. By resolution of forces it will appear, that when the animal bends its tail, the tendency is to draw it *backward*. It is impelled *forward* by rapidly jerking the tail to its first natural position. The tail is bent *slowly*, and unbent *quickly*; and since the resistance varies nearly as the *square* of the velocity, the force forward is much greater than the force backward. This shows how very material in the question it is

 that

that the velocity of any machinery used should be considerable, a small difference in that producing a considerable difference in the resulting force. The principle is applied in swimming.

4. One writer recommends a series of explosions with gunpowder. This is very crude. It would indeed be possible to gain some power by creating a vacuum in front of the machine, and by impelling the air drawn from the front through a pipe, to rush out behind. This might be effected by connecting the centre of the pipe with a tube and piston. In raising the piston, the air will rush through the pipe from the front, while it is prevented by a valve from passing in through the other part; on depressing the piston, the air is impelled through the pipe behind, while another valve prevents its regress to the front. The principle differs but little from that of the blowing engine and common bellows.

5. Some have thought it possible to tame the eagle, or imitate his wings; while others, as Blanchard, made an ineffectual attempt with valved oars. (See vol. xlix. p. 197, and lxi. p. 6, of this Journal.)

6. But let us look philosophically on the matter. In what cases is the air used as an *agent;* and can we by *reverting the operation* use similar means to re-act on the air itself? It is used to fill the sails of ships, and it has conversely been proposed to push out canvass against the air: some power so gained might be retained, by withdrawing the canvass in a folded form, or by valves, or by protruding it with considerable velocity, and withdrawing it slowly.

7. Wind is used to turn the sails of mills, and in so acting swells them into curves investigated by mathematicians. Would it be possible, by turning sails, *inflexibly* maintaining the *form so produced,* in an *opposite* direction, to re-act upon the air, as it had acted upon the sails? These sails would be fixed behind and turned by machinery.

8. We might combine the principle of the steam-boat paddles with that of the horizontal windmill. The paddles gain their power by being only partly immersed in the water, so that in returning they do not act at all. In the horizontal windmill this difficulty has been and may be obviated by a great number of ingenious contrivances; for some of which see Dr. Brewster's *Encyclopædia,* "Mechanics," and its references, p. 570. Those paddles may be fixed on the edges of the platform connecting our two balloons, the sails of the paddles being so constructed as to furl themselves in returning. There is great advantage in making our power operate by a rotary motion,

motion; and we should remember that an impulse once given will maintain itself long with little support. Virgil says of the dove:

"Celeres neque commovet alas."

Having enumerated such suggestions as do not seem utterly groundless, and which may separately or in combination supply materials for the speculations of others, I will add a hope that the Society of Arts may draw the attention of scientific persons to a full investigation of the question. Yours &c.

SEPTIMUS.

QUERIES.

1. Does not the velocity of an aërial current generally diminish very rapidly as it approaches the earth, and should not an aërostatic machine be kept low in the air, except when the current is favourable?—*Vide* Principia, book ii. last sect.

2. Since the resistance varies nearly as $a v^2 - b v$, v being the velocity; and since the power acting upon the air may move with much greater velocity than that produced in the balloon's motion,—will not power be gained by acting in the lower and denser strata of the atmosphere?

3. Must not the line of direction of any force, to be applied with a maximum effect, pass through the centre of pressure of the system? And what must be the position of a plane connecting two balloons, that a force acting in the direction of the plane may pass through the centre of pressure of the whole system, one balloon being supposed to precede?

4. Let a fan be wound spirally about a rod, like an Archimedes' screw, and let the rod turn in the air on its axis with a uniform velocity,—will the rod acquire any other motion in addition to its motion of rotation, and in what direction?

XXII. *Notices respecting New Books.*

PROPOSALS have been issued for publishing by subscription, in about fifty quarterly parts, *Species Conchyliorum:* or, Descriptions of all the known species of Recent Shells. By G. B. Sowerby, F.L.S. Illustrated with coloured plates, by J. D. C. Sowerby, F.L.S.—The temporary possession of the celebrated Tankerville Collection, Messrs. Sowerby observe, will enable them to secure drawings and descriptions of many shells that could not otherwise be easily obtained: this, in addition to their own private collections, and the immense number of species contained in the collection late the property of Mr. George Humphrey, with the access they have to the cabinets of many of their friends, will enable them to render this work by far the most complete of its kind.

 Recently

Recently published.

Nos. III. and IV. of the Zoological Journal.

The sixth edition of Dr. Paris's Pharmacologia; in which is introduced a revolving scale, termed the Medical Dynameter, showing the absolute and relative strength of the different preparations of medicine: in 2 vols. 8vo.

Traité de Chimie Elementaire, théorique et pratique; par J. L. Thenard. 5 vols. 8vo; fourth edition, 1824.

Sur la Distribution de la Chaleur dans un anneau homogène et d'une epaisseur constante lorsque la température du lieu où il est placé varie d'un point à un autre; par M. Poisson. (*Connaissance de Temps*, 1826.)

Plantes Cryptogames du Nord de la France; par J. B. H. J. Desmazières. Fasc. I. Lisle 1825. Treuttel and Würtz.

Second Discours sur la Géomêtrie et la Mécanique appliquées aux Arts.—Résumé général des Applications de la Géométrie: Discours prononcé dans l'amphithéâtre du Conservatoire des arts et métiers, le 22 Decembre 1824; par Charles Dupin, de l'Académie des Sciences. Paris 1825. 8vo.

Systema Algarum adumbravit E. A. Agardh, Professor. Lund. 1824. 1 vol. 12mo.

Pomona Italiana, &c. The Pomona of Italy, or a Treatise on the Fruit-Trees of that country; by George Gallesio. Pisa, 1817—1824.

ANALYSIS OF PERIODICAL WORKS ON NATURAL HISTORY.

Curtis's British Entomology. No. 12.

Pl. 47. *Pogonus Burrellii*, a very pretty species belonging to this genus, (which is *Raptor* of Megerle) discovered in abundance in Norfolk by the Rev. Mr. Burrell, from whom it receives its name.—Pl. 48. *Pontia Daplidice*, the only figure that has ever been given of a British specimen of this extremely rare insect, those appearing in other works having been drawn from foreign specimens.—Pl. 49. *Trichiosoma laterale*, a species never before figured of a genus formed by Dr. Leach, from the Fabrician genus *Cimbex*. — Pl. 50. *Limnobia ocellaris*, a very pretty species, which, although described by Linnæus, has never before been figured. There are several species with the wings ornamented with pretty ocellated spots; from all of which this may be distinguished by the single black annulation round each femur.

Having here concluded his first volume, the author thus briefly reviews its contents. "It has been found necessary to establish four new genera, *Acanthosoma*, *Sarrothripus*, *Peronea*, and *Hæmobora;* five more are now for the first time recorded as British, viz. *Pogonus*, *Omaseus*, *Xyela*, *Ibalia*, and *Eumenes*. —Figures of fourteen of the species it is believed have never before been published in any work, viz. *Pogonus Burrellii*, *Omaseus aterrimus*, *Aphodius villosus*, *Hydrometra Stagnorum* (with wings), *Velia Rivulorum*, *Peronea ruficostana*, *Cimbex decem-maculata*, *Trichiosoma laterale*, *Eumenes atricornis*, *Ctenophora ornata*, *Limnobia ocellaris*, *Pachygaster Leachii*, *Anthrax ornata*, and

and *Hæmobora pallipes;* of the remaining thirty-six species, only eleven of them have been figured in any British work.—It is not only hoped that these novelties will be sufficient to recommend this volume to the student, but likewise render it useful to those more advanced in the science both at home and abroad. The author also trusts that those who have purchased the volume for its embellishments, or for the figures of the British plants which it contains, will not have been disappointed in the selection that has been made."

The execution of the work, as far as it has hitherto proceeded, abundantly justifies the favourable opinion which its first numbers led us to express; and we sincerely hope that Mr. Curtis's meritorious labours as an artist and naturalist will meet with the encouragement which they richly deserve.

Vol. II. Nos. 13 & 14.

Pl. 51. *Platypus cylindrus*, a singular insect belonging to Latreille's Class *Heteromera*, but which has a fifth minute joint at the base of the terminal one, as Mr. W. S. MacLeay has lately proved many of the *Curculionidæ* to have, and which he has lately demonstrated before the Zoological Club.—Pl. 52. *Onthophagus Taurus.* A male of this curious insect having been taken last autumn in the New Forest, a figure of it is given for the first time in any British work.—Pl. 53. *Ægeria Ichneumoniformis* Fab.: *Vespiformis* Haworth; a very rare species of the natural and beautiful genus *Ægeria*, which is rendered remarkable by the ocelli, which, as the author observes, is an additional proof of analogy between this Order and *Trichoptera*. He has also revised the genus, and settled the true names and synonyms, which in this country until now were in great confusion.—Pl. 54. *Lophyrus Pini.* Both sexes of this beautiful and singular insect are figured, and an interesting account of them given from the learned De Geer.—Pl. 55. *Melasis buprestoides;* a figure of this curious and interesting insect has never before appeared in any British work, and the only dissections that we know of the "instrumenta cibaria," which are given by Olivier, are very far from accurate.—Pl. 56. *Eulepia Cribrum.* We know of no figure of this extremely rare insect except in the work of Hubner, of which probably there are not more than two complete copies in the kingdom: the pectinated antennæ separating it from *Lithosia*, and the general habit from *Eyprepia*, the author has divided this and *Bombyx graminica* from them, and established them as a new genus; and we think that dissections of such rare insects as have never been attempted before in any work upon this beautiful Order, must be very interesting to the Lepidopterist.—Pl. 57. *Leptocerus ochraceus*, a new species of this pretty genus. The dissections here again must be very valuable to the investigator of nature; as there are, we believe, no dissections of this family, excepting the *Labrum* in Savigny's *Mémoires sur les Animaux sans Vertèbres.*—Pl. 58. *Cryptus pallipes.* The male of this curious insect is here given, which is now figured for the first time, having been first described by Dr. Leach in the Zoological Miscellany.

Lectures on the Phænomena and History of Igneous Meteors and Meteorites.

E. W. Brayley junior, A.L.S., will shortly commence, at the Russel Institution, Great Coram-street, a Course of Lectures on the Phænomena and History of Igneous Meteors and Meteorites; to be illustrated by a series of transparent diagrams of Meteors, an extensive collection of Meteorites, and various experiments in Chemistry and Natural Philosophy.

THE

THE LATE ALEXANDER TILLOCH, LL.D.

It is with feelings of deep emotion that we have to announce to our readers the death of a gentleman with whose name and talents they have long been acquainted—Dr. Alexander Tilloch, the founder and editor of the Philosophical Magazine, the second monthly periodical work wholly devoted to scientific subjects published in this country; the first having been Mr. Nicholson's Philosophical Journal, which some years since merged into this work.

We have neither the leisure nor would our feelings permit us to do justice to the memory of our late much-esteemed friend and coadjutor; we shall however lose no time in preparing such a Memoir of him as his character and talents demand, and which will be given in a future Number of the Philosophical Magazine:—our present notice therefore can only be considered as a brief obituary.

Alexander Tilloch was a native of Glasgow, where he was born on the 28th of February 1759. After receiving that liberal education which in Scotland is so much more accessible than in England, inured from his earliest life to a habit of thinking for himself, possessing an inquisitive mind, and imbibing an ardent thirst for knowledge, he devoted much of his attention to the art of printing, in which he conceived much improvement remained to be made. As he was not bred a printer himself, he had recourse to Mr. Foulis, printer of the University of Glasgow, to whom he applied for types to make an experiment in a new process, and that nothing less than the art of stereotype printing: the experiment succeeded, and Mr. Foulis, who was a very ingenious man, became so convinced of its practicability and excellence, that he entered into partnership with him in order to carry it on. They took out patents in both England and Scotland, and printed several small volumes from stereotype plates. A few years afterwards Dr. Tilloch discovered that although he had invented stereotype printing, yet he was but a second inventor, and that the art had been exercised by a Mr. Ged of Edinburgh, jeweller, nearly fifty years before. This circumstance, if it did not disgust Dr. Tilloch, made him think less of his discovery; and soon after he left Glasgow for London, where he became one of the proprietors of the Star evening newspaper;

but even the avocations of a daily journal, and the political vortex into which all connected with it are unavoidably driven, could not divert his mind from its favourite pursuits. He saw with regret that while in London there were a host of periodicals, there was but one in which the man of science could embody his own discoveries or become acquainted with those of others; he therefore projected and commenced the Philosophical Magazine, which has now reached its 65th volume. The example was soon afterwards followed; but although there are now several works of a similar description, the Philosophical Magazine has continued to maintain its high character. To this the philosophical acquirements of the Editor, who possessed an extensive knowledge of many departments of physical science, were in a great degree conducive; and various papers by himself in the earlier volumes are by no means the least interesting of their contents. During the last three years, however, the ravages of the disorder which has terminated in his death, disabled him from taking an active part in conducting the work.

Dr. Tilloch devoted much of his valuable time to the Steam-engine, and had a large share in suggesting and maturing the improvement on what is called Woolf's Engine. The ruling passion may be said in Dr. Tilloch to have been strong almost even in death; for he had entered a new patent for a steam-engine only a fortnight before death closed his eyes, and the world lost a man who had devoted a long life to the advancement of science. This melancholy event took place at his house in Barnsbury-street, Islington, on the 26th of January last.

In private life Dr. Tilloch was amiable; in conversation acute, intelligent, and communicative; few persons possessed a clearer understanding or a warmer heart. We have already stated that Dr. Tilloch was one of the proprietors of the Star newspaper, and for many years he took an active share in its management; for the last five years, however, the editing has been confided to other hands, and the opportunities that a long and protracted sickness enabled him to devote to study were appropriated to science, in the promotion of which he was always ardent and persevering.

Dr. Tilloch was a member of several literary and scientific societies, and few individuals had stronger claims to those distinctions.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 3.—THE reading of Dr. Kidd's paper On the anatomy of the mole-cricket, was concluded; and a notice read, On the nerves of the human placenta; by Sir E. Home, Bart. V.P.R.S.

Feb. 10.—A paper was read, entitled Notice of the *Iguanodon*, a fossil herbivorous reptile found in the sandstone of Tilgate Forest; by Gideon Mantell, F.L.S. Communicated by Davies Gilbert, Esq. V.P.R.S.

Feb. 17.—A paper was read, entitled An experimental inquiry into the nature of the radiant heating effects from terrestrial sources; by the Rev. B. Powell, M.A. F.R.S.

Feb. 24.—Dr. John Thomson, F.R.S., communicated a paper, On the materno-fœtal circulation, by David Williams, M.D.; part of which was read, and the remainder postponed to the next meeting.

LINNÆAN SOCIETY.

Feb. 1.—A paper was read, On the structure of the tarsus in the tetramerous and trimerous Coleoptera of the French entomologists. By W. S. MacLeay, Esq. A.M. F.L.S.

It is the object of this communication to show that all Coleoptera are typically pentamerous; that the tarsal system not only violates many obvious relations both of affinity and analogy, but is not even founded in truth; the majority of insects classed as tetramerous having in reality not *four* but *five* joints: as for instance, the Linnæan genera *Curculio*, *Cerambyx*, and *Chrysomela*. It is also shown that all the trimerous insects of the French entomologists are at least tetramerous.

Feb. 15.—A collection of New Holland birds was exhibited, presented by Mr. Icely.

M. C. S. Kunth, of Berlin, and Professor Fr. A. Bonelli, of Turin, were severally proposed as candidates to fill a vacancy which had occurred among the foreign members.

The reading of Messrs. Sheppard and Whitear's paper On the birds of Norfolk and Suffolk, and of Dr. Hamilton's Commentary on the *Hortus Malabaricus*, was continued.

GEOLOGICAL SOCIETY.

Jan. 21.—A paper was read, entitled On the Fresh-water formations recently discovered in the environs of Sete (Cette), at a short distance from the Mediterranean, and below the level of that sea; by M. Marcel de Serres, Prof. of Min. and Geol. to the Faculty of Sciences of Montpellier.

The

The fresh-water formations described in this communication have been examined by means of several wells sunk at about the distance of three-quarters of a mile and a mile and a half from the Mediterranean, near Sete, in the South of France.

A detailed account is given of the several strata passed through in the three different wells, and of the organic remains which they contained.

The strata are for the most part parallel and nearly horizontal.

From the sections it appears there are two fresh-water formations with an intervening formation of marine origin. The strata of the upper fresh-water were found to vary from about 30 to 40 feet in thickness; those of the lower from 13 to 28 feet: the latter being sometimes lower than the present level of the Mediterranean.

The marine beds which are interposed, are from 10 to 11 feet thick.

The fresh-water strata are composed of numerous alternating calcareous and argillaceous marls, and compact limestones; and their organic remains consist of a few bones of land quadrupeds much decayed, a variety of fresh-water and terrestrial shells, the latter in the greatest abundance; the shells differing in species but not in genera from the present inhabitants of the same country; and lastly, some traces of vegetables, chiefly reeds.

The marine formations contain *ostreæ*, *cerithia*, &c.:—A complete list is added of the organic remains:—from the state of preservation in which the fresh-water shells are found, Mons. Marcel de Serres infers that they lived and were deposited where they are now found; and from the resemblance of those occurring in the upper and lower fresh-water beds, he concludes that the periods at which these two formations were deposited were not very remote from each other.

The author considers all these formations to be more recent than the *Calcaire Grossiere*, and ascribes the alternations of marine and fresh-water strata to a return of the sea; such a suppostion being rendered the more probable by the neighbourhood of the Mediterranean, where similar returns are still known to take place.

On February 4, being the Anniversary of the Society, the following members were chosen as Officers and Council for the ensuing year:—

President: Rev. William Buckland, F.R.S. *Prof. Geol. and Min. Oxford.*—*Vice-Presidents:* Sir Alexander Crichton, M.D. F.R. & L.S. *Hon. Memb. Imp. Acad. St. Petersburgh;* William Henry Fitton, M.D. F.R.S.; Charles Stokes, Esq. F.R.A. &

L.S.; Henry Warburton, Esq. F.R.S.—*Secretaries:* Charles Lyell, Esq. F.L.S.; George Poulett Scrope, Esq.; Thomas Webster, Esq.—*Foreign Secretary:* Henry Heuland, Esq.—*Treasurer:* John Taylor, Esq.—*Council:* Hon. Henry Grey Bennet, M.P. F.R.S. & H.S.; Richard Bright, M.D. F.R.S.; Sir Henry Bunbury, Bart.; Henry Burton, Esq.; William Clift, Esq. F.R.S.; Henry Thomas Colebrooke, Esq. F.R.S. L. & E. F.L. & Asiat. S.; George Bellas Greenough, Esq. F.R. & L.S.; Thomas Horsfield, M.D. F.L.S.; Gideon Mantell, Esq. F.L.S.; Hugh Duke of Northumberland, K.G. F.H.S.; William Hasledine Pepys, Esq. F.R.S. L.S. & H.S.; John Vetch, M.D.

HORTICULTURAL SOCIETY.

Feb. 1.—The silver medal of the Society was presented to Mr. George Lindley, a corresponding member of the Society, for his paper on a classification of peaches, printed in the Transactions.

The following papers were read:—Upon the apparently beneficial effects of protecting the stems of fruit-trees from frost in early spring. By Thomas Andrew Knight, Esq. F.R.S. &c. President.—On the management of hot-house flues, so as to keep up an equal temperature during the night. By the Rev. George Swayne, corresponding member of the Society.

Feb. 15.—The following paper was read:—On forcing established cherry-trees under glass. By Mr. Thomas Allen.

ASTRONOMICAL SOCIETY.

Feb. 11.—The fifth Annual General Meeting of the Society was this day held at the Society's rooms in Lincoln's Inn Fields, for the purpose of receiving the Report of the Council upon the state of the Society's affairs, electing Officers for the ensuing year, &c. &c.

The President, H. T. Colebrooke, Esq. in the chair.

The Report, which was read by Dr. Gregory, and ordered to be printed for distribution amongst the members, commenced by expressing the gratification felt by the Council on witnessing the growing prosperity of the Society, and the increasing evidence of the utility of its institution. It proceeded to state that, for the purpose of still further alleviating the labour of the practical astronomer (the Society having already published, in part 2. vol. i. of its Memoirs, tables for facilitating the computation of the apparent places of 46 principal stars), the Council had deemed it desirable that tables of precession, aberration, and nutation should be computed, embracing, 1st, all stars above the 5th magnitude; 2d,

2nd, all stars to the 6th magnitude inclusive, whose declination should not exceed 30°; and 3d, all stars to the 7th magnitude inclusive, within 10° of the ecliptic,—and that a considerable portion had already been computed under the superintendence of Mr. Baily and Mr. Gompertz, and would be forthwith published, accompanied by an explanatory preface drawn up, at the request of the Council, by Mr. Baily. The Report then noticed, in terms of well merited panegyric, the very valuable collection of astronomical tables lately published by Dr. Pearson, the Treasurer; and it will be no little gratification to the scientific world to be informed that the tables constitute only a part of a comprehensive treatise on Practical Astronomy upon which Dr. Pearson is still engaged. It then adverted to the visit of Mr. Herschel (the foreign secretary) to Italy and Sicily, from which, besides other very considerable benefits, the Society had derived increased facilities of communication with the continental astronomers, nearly the whole of whom the Society had now the honour of numbering amongst its associates. The Report conveyed a just tribute of respect to the memory of the late Major-general John Rowley of the Royal Engineers, F.R.S., and a member of this Society, of which he was a cordial friend from its commencement. After alluding to the acquired stability and acknowledged utility of the institution, which might justify an application to the Crown for a Charter of incorporation, the Report stated that the expediency of such an application would most probably engage the consideration of the Council for the ensuing year. It concluded by strenuously advising concert and co-operation—observing, that though much had been done to advance astronomical science and much was in progress, much yet remained to be done. "On the retrospect of the past, however, your Council derive confidence with regard to the future. Let the zeal, activity and talent of the Members and Associates for the next ten years but keep pace with the efforts of the last five, and the most interesting, brilliant, and beneficial results may unhesitatingly be anticipated."

A list of the papers read at the ordinary meetings, followed by a numerous list of benefactors and a gratifying statement of the Society's finances was then read, after which the Members present proceeded to ballot for the Officers for the ensuing year, when the following were declared to have been duly elected.

President: Francis Baily, Esq. F.R.S. & L.S.—*Vice-Presidents:* Charles Babbage, Esq. M.A. F.R.S. L. & E.; Rev. John Brinkley, D.D. F.R.S. Pres. R.I.A. *And. Prof. Ast. Univ. of Dublin;* Davies Gilbert, Esq, M.P. V.P.R.S. & F.L.S.; George Earl of Macclesfield, F.R.S.—*Treasurer:* Rev. William

Pearson, LL.D. F.R.S.—*Secretaries:* Olinthus G. Gregory, LL.D. *Prof. Math. Roy. Mil. Acad. Woolwich;* John Millington, Esq. F.L.S. *Prof. Mech. Phil. Roy. Inst.* — *Foreign Secretary:* J. F. W. Herschel, Esq. M.A. F.R.S. L. & E. — *Council:* Captain F. Beaufort, R.N. F.R.S.; Major T. Colby, *Roy. Eng.* LL.D. F.R.S. L. & E.; Henry T. Colebrooke, Esq. F.R.S. L. & E. & L.S.; Bryan Donkin, Esq.; Rev. William Dealtry, B.D. F.R.S.; Benjamin Gompertz, Esq. F.R.S.; Stephen Groombridge, Esq. F.R.S.; Edward Riddle, Esq.; Richard Sheepshanks, Esq. M.A.; Edward Troughton, Esq. F.R.S. L. & E.—The Society afterwards dined together at the Freemasons Tavern, to celebrate their fifth anniversary.

ROYAL ACADEMY OF SCIENCES OF PARIS.

Dec. 6.—The Minister of the Interior solicited the Academy to nominate a candidate for the Professorship of the cultivation and naturalization of exotic plants, at the *Jardin du Roi*, vacant by the death of M. Thouin.—M. Loiseleur de Longchamps communicated a supplement to his memoir on the means of obtaining several crops of silk in the year.—Dr. Villermet continued the reading of his memoir on the comparative mortality of the middle and poor classes of people.—M. Jomard communicated an extract from a letter dated September 27, 1824, relating to M. de Beaufort's expedition into the interior of Africa.—M. le Baron Cagniard de la Tour read a memoir, entitled Reflections on vibrating cords; experiments in support of those reflections.—M. de Ferussac read a notice on the animal of the genus *Argonauta*.

XXIV. *Intelligence and Miscellaneous Articles.*

THE BRITISH MUSEUM—MR. GOODWYN'S MANUSCRIPTS.

THOSE who are interested in mathematical computations, and the tabulation of their results for practical purposes, will learn with pleasure that the curious and extensive Tables of the late Henry Goodwyn, Esq. of Blackheath, have, by the advice of Dr. Gregory, Professor of Mathematics in the Royal Military Academy, been deposited by Mr. Goodwyn's family in the library of the British Museum.—The following copy of Dr. Gregory's account of the general nature of the manuscripts will serve to convey the requisite information to our readers.

"The late Henry Goodwyn, Esq. of Blackheath, being for several years kept by ill health from the more active pursuits of life, devoted a great portion of his time to the most laborious computations, many of them relating to topics and leading to results that are exceedingly curious and interesting. Some of these he applied to practical inquiries relative to interest

terest and annuities; others to the determination of powers and roots; others to the reduction and comparison of weights and measures, whether British or foreign, and to the formation of a general system; and others he rendered applicable to the rules of mensuration and to still higher inquiries among mathematicians.

"In the pursuit of these researches he developed various interesting properties indicative of the mutual connexion between circulating decimals and prime numbers, entering either simply or compositely into the denominators of fractions respectively equivalent to those decimals; of which properties some have been long known to mathematicians, while others had almost, if not altogether, escaped their notice. A few of these are explained in the Appendix to the quarto pamphlet to which this paper is attached *; and in that appendix *one* of Mr. Goodwyn's ingenious improvements in computation is described and applied.

"The results of his persevering and long-continued labours, have, as yet, been only very partially laid before the public in a few detached pamphlets, volumes, &c.;—copies of all which are herewith transmitted. But his two works of greatest labour, the one denominated "A Table of complete decimal quotients," and the other "A Tabular series of decimal quotients for all the proper vulgar fractions," of which, when in their lowest terms, neither the numerator nor the denominator is greater than 1000, still remain in manuscript. The former of these is comprised in four folio volumes of manuscript, and lettered "Table of complete quotients."

"Mr. Goodwyn had finished their computation, and by subsequent calculations had nearly, if not entirely, verified the correctness of the whole. He had also advanced considerably in the computation of the "Tabular series," the results being entered and duly arranged in five volumes large quarto; in the last of which, however, the *platform* of his labours is alone exhibited.

"A comparison of the respective manuscripts with the two royal octavo printed volumes entitled "Table of the circles and tabular series," and with the quarto pamphlet to which this is annexed, will enable any competent judge to appreciate the extent of these classes of Mr. Goodwyn's labours, their utility, and the comparative value of those portions which still remain unpublished.

"Mr. Goodwyn's family, anxious to consign these manu-

* Entitled "The first Centenary of a concise and useful Table of complete Decimal Quotients," with a specimen of "A tabular Series," &c.

scripts

scripts of their revered relative to some institution where they may be occasionally consulted by the friends and promoters of mathematical science, do now, with the consent of the trustees of the British Museum, deposit them in the library of that magnificent national institution.

Royal Military Academy, Woolwich, Nov. 1824. OLINTHUS GREGORY."

NEWLY-DISCOVERED ISLAND.

A new island in the Southern Ocean was discovered last July by Capt. Hunter, of the Donna Carmelita. It is described as "entirely composed of lava, in some places almost a metal. It lies in the latitude of 15° 31 S.; and longitude 176° 11 E. by sun and moon, brought up by chronometer for four days previous." It is called Onacuse, or Hunter's Island. The inhabitants, who came off in canoes, manifested a friendly disposition; and the cutter was sent ashore in charge of the first officer, who had an interview with the king, and trafficked with the natives on very friendly terms for provisions. They are about the colour of Malays, but have more of the European features. Their canoes are very handsome, not unlike those of Ceylon, and ornamented with shells.

EXPEDITION FROM MISSOURI TO MEXICO.

A statement of facts has been lately presented to Congress from Mr. Augustus Storrs, of New Hampshire, in relation to the origin, present state, and future prospect of trade and intercourse between the Valley of the Mississippi and the internal provinces of Mexico, which was ordered to be printed and referred.

Mr. S. has been one of a caravan of eighty persons, 156 horses, and 23 waggons and carriages, which had made the expedition from Missouri to Santa Fé (of New Mexico), in May and June last. His account was full of interest and novelty. It sounded like romance to hear of caravans of men, horses, and waggons, traversing with their merchandize the vast plain which lies between the Mississippi and the Rio del Norte. The story seemed better adapted to Asia than to North America. But, romantic as it might seem, the reality had already exceeded the visions of the wildest imagination. The journey to New Mexico, but lately deemed a chimerical project, had become an affair of ordinary occurrence. Santa Fé, but lately the *Ultima Thule* of American enterprise, was now considered as a stage only in the progress, or rather a new point of departure to our invincible citizens. Instead of turning back from that point, the caravans broke up there, and the subdivisions

divisions branched off in different directions in search of new theatres for their enterprise. Some proceeded down the river to the *Passo del Norte*, some to the mines of Chihuahua and Durango, in the province of New Biscay; some to Sinera and Sinatoa, on the Gulf of California; and some, seeking new lines of communication with the Pacific, had undertaken to descend the western slope of our continent through the unexplored regions of the Multnomah and Buenaventura. The fruit of this enterprise, for the present year, amounted to one hundred and ninety thousand dollars in gold and silver bullion, and coin, and precious furs; a sum considerable in itself in the commerce of an infant state, but chiefly deserving a statesman's notice, as an earnest of what might be expected from a regulated and protected trade. The principal article given in exchange is that of which we have the greatest abundance, and which has the peculiar advantage of making the circuit of the Union before it departs from the territories of the Republic—cotton, which grows in the south, is manufactured in the north, and exported from the west.

RAIL-ROADS.

On a well made road a horse will draw a load of one ton, in a cart weighing 7 cwt. at the rate of two miles an hour—(Leslie's Elements, p. 253). The whole strength of the horse is exerted in overcoming the friction. On such a road, therefore, a force of traction of 100 pounds moves a weight of 3,000 pounds, or the friction is 1-30th part of the load (the cart included).

On a rail-way of the best construction it has been shown in a former paper, that a horse travelling at the same rate of two miles an hour draws 15 tons, including the vehicles. In this case then a power of traction of 100 pounds moves a weight of 33,600 pounds; the friction of course is 1-336th part—or in round numbers 1-300th part of the load.

On a canal, a horse travelling at two miles an hour draws 30 tons in a boat weighing probably 15 tons*. Reducing the ton to 2,000 pounds for the sake of round numbers, as in the last calculation, we find here that a power of traction of 100 pounds moves a mass of 90,000 pounds—or the resistance which the water opposes to the motion of the vessel is equal to 1-900th part of the load or entire weight. At sea, where the water-way is of unlimited breadth, the resistance is probably one-third less; but as a compensation for this, when steam-

* Boats in some cases carry only 15 or 20 tons, in others 35 (as the coal boats on the Union Canal); but in the one case they travel quicker, and in others slower, than the rate mentioned.

power

power is employed, there is probably a loss of one-third in consequence of the disadvantageous mode of its application.

We see then that the effect produced by the draught of a single horse is ten times as great upon a rail-way, and thirty times as great upon a canal, as upon a well made road. Yet a rail-way costs only about three times as much as a good turnpike road*, and a canal about nine or ten times; and the expense of keeping the rail-way and canal in repair is probably less in proportion to the original outlay than in the case of a road. It is obvious, then, that were rail-ways to come into general use, two-thirds or more of the expense of transporting commodities would be saved. With regard to the comparative advantages of canals and rail-ways, so far as the present facts go, we may observe, that if a horse-power effects three times as much upon a canal as upon a rail-way, the canal costs about three times as much, and will of course require nearly the same rates or *dues* per ton to make the capital yield the same interest.

But here it is of great importance to recollect that this computation refers solely to a velocity of *two miles an hour.* If the friction which impedes the motion of a car or waggon and the resistance which the water offers to the progress of a ship were governed by the same laws, the same conclusions would hold true whatever the velocity might be. But this is far from being the case, as we shall presently see. In illustrating this point, it will be convenient, instead of estimating effects by the variable measure of a horse-power, to refer to a determinate and constant force of traction of a given amount. We shall therefore assume, that the body to be moved is urged forward by force exactly equivalent to a weight of 100 pounds suspended over a pulley at the end of the plane on which it moves.

First, with regard to the motion of a body in water. It is deduced from the constitution of fluids, and confirmed by experiment, that the resistance which a floating body encounters in its motion through the fluid is as the square of the velocity†. Now, taking as a basis the known effect of force of traction of 100 pounds at two miles an hour, let us ascertain what force would move the same body at a greater velocity. On a canal, or arm of the sea, we have seen that a body weighing 90,000 pounds is impelled at the rate of two miles an hour by a force of 100 pounds; therefore, to move the same body

* In Mr. Telford's estimates for portions of new road between Edinburgh and Wooller, we find the expense to be from one thousand to one thousand one hundred pounds per mile, including the price of the ground.

† See Playfair's Outline, i. 198; Leslie's Elements, sec. vii. article "*Resistance,*" Encycl. Brit.

At 4	miles an hour, will require		....	400 pounds.
At 6	ditto	ditto	....	900
At 8	ditto	ditto	....	1600
At 12	ditto	ditto	....	3600

Or conversely:—

100 pounds moves	90,000 pounds	at 2	miles an hour.
or	22,500	at 4	ditto
or	10,000	at 6	ditto
or	5,620	at 8	ditto
or	2,500	at 12	ditto

Hence we see that when we have to contend with the resistance of water, a great increase of power produces but a small increase of velocity. To make a ship sail *three* times faster, for instance, we must employ *nine* times the power; and to make her sail *six* times faster, we must employ no less than *thirty-six* times the power. Let us suppose, for example, that it were required to determine, since one horse draws a boat loaded with thirty tons at two miles an hour, how many horses would draw the same boat at four miles. We find, first, that since the boat is to move *two* times as fast, it will require *four* times the absolute amount of power, or 400 pounds. But a horse moving at four miles an hour, pulls only with a force of 64 pounds. Of course, it would require six horses to exert a power of 400 pounds, and move the boat at the rate proposed.

Let us now see what amount of power will produce corresponding effects upon a rail-way. And before we make more particular inquiry, let us suppose that the retardation occasioned by friction, instead of increasing as the square of the velocity like the resistance of a fluid, increases in the simple ratio of the velocity. We have seen, then, that a force of traction of 100 pounds upon a level rail-way, moves a body weighing 30,000 pounds at the rate of two miles an hour. We may hence calculate the effect produced by any greater amount of power:—

30,000 lbs. are moved	at 2	miles an hour	by a power of		100 lb.
	at 4	miles	...	by ...	200 lb.
	at 6	miles	...	by ...	300 lb.
	at 8	miles	...	by ...	400 lb.
	at 12	miles	...	by ...	600 lb.

Or conversely:—

A power of 100 pounds moves	30,000 lb.	at 2	miles per hour.
or	15,000 lb.	at 4	
or	10,000 lb.	at 6	
or	7,500 lb.	at 8	
or	5,000 lb.	at 12	

Hence we see that, though a moving force of 100 pounds produces three times as great an effect upon a canal as upon a rail-way at 2 *miles an hour*, this superiority of the water conveyance is lost if we adopt a velocity at 6 miles an hour; and at all greater velocities the same expenditure of power will produce a greater effect upon a rail-way, than upon a canal, a river, or the sea.

This calculation proceeds on the hypothesis that the friction increases in the simple ratio of the velocity. Such was the opinion of Ferguson, Musschenbroeck, and some other writers; but the more recent and accurate experiments of Coulomb and Vince have overthrown this doctrine, and established conclusions extremely different, of which the following is an abstract*:—

1. The friction of iron sliding on iron is 28 per cent of the weight, but is reduced to 25 per cent after the body is in motion.

2. Friction increases in a ratio nearly the same with that of the pressure. If we increase the load of a sledge or carriage four times, the friction will be nearly, but not quite, four times greater.

3. Friction is nearly the same whether the body moves upon a small or greater surface; but it is rather less when the surface is small.

4. The friction of rolling and sliding bodies follows nearly, but not precisely, the same law as to velocity; and that law is, that *the friction is the same for all velocities.*

It is with this last law only we have to do at present; and it is remarkable that the extraordinary results to which it leads, have been, so far as we know, entirely overlooked by writers on roads and rail-ways. The results, indeed, have an appearance so paradoxical, that they will shock the faith of practical men, though the principle from which they flow is admitted without question by all scientific mechanicians.

First. It follows from this law that (abstracting the resistance of the air) if a car were set in motion on a level rail-way, with a constant force greater in any degree than is required to overcome its friction, *the car would proceed with a motion continually accelerated, like a falling body acted upon by the force of gravitation;* and however small the original velocity might be, it would in time increase beyond any assignable limit. It

* Leslie's Elements, p. 188, &c.; Playfair's Outlines, i. 88, &c.; *Journal de Physique*, 1785; Philosophical Transactions, 1785. Dr. Brewster has given the results of Coulomb's experiments in a tabular form, in the article "*Mechanics*" in his Encyclopædia.

is only the resistance of the air (increasing as the square of the velocity) that prevents this indefinite acceleration and ultimately renders the motion uniform.

Secondly. Setting aside, again, the resistance of the air (the effects of which we shall estimate by and by), *the very same amount of constant force which impels a car on a rail-way at 2 miles an hour, would impel it at 10 or 20 miles an hour* if an extra force were employed at first to overcome the *inertia* of the car and generate the required velocity. Startling as this proposition may appear, it is an indisputable and necessary consequence of the laws of friction. In fact, assuming that the resistance of the air were withdrawn, if we suppose a horizontal rail-way made round the globe, and the machine (supplied with a power exactly equivalent to the friction) to be placed on the rail-way, and launched by an impulse with any determinate velocity, it would revolve for ever with the velocity so imparted, and be in truth a sort of secondary planet to our globe.

Now, it would be at all times easy (as we shall afterwards show) to convert this accelerated motion into a uniform motion of any determinate velocity; and from the nature of the resistance, a high velocity would cost almost as little, and be as easily obtained as a low one. For all velocities, therefore, above four or five miles an hour, rail-ways will afford facilities for communication prodigiously superior to canals or arms of the sea.—*Scotsman*, Dec. 8, 1824.

WEIGHTS AND MEASURES.

Copy of a Letter from the Commissioners of Weights and Measures, dated 14th January 1825, to J. C. Herries, Esq., Secretary of the Treasury, transmitting a Report of the Progress made in the preparation of the Models of the new Weights and Measures.

London, January 14, 1825.

SIR—I am directed by the Commissioners of Weights and Measures to transmit to you, for the information of the Lords Commissioners of His Majesty's Treasury, the inclosed Report from Capt. Kater, stating the progress which he has made in the preparation of the models of the new weights and measures, in pursuance of the directions contained in your letter of the 13th of July 1824, inclosing a copy of a Treasury minute, dated the 29th of June 1824, respecting the steps necessary to be taken for carrying into effect the Act 5th Geo. IV., for ascertaining and establishing uniformity of weights and measures.

In consequence of the delay which unfortunately has occurred from the difficulties which have been experienced in the

the construction of the new bushel measure, I am further directed to submit to you, for the consideration of the Lords Commissioners of His Majesty's Treasury, the propriety of bringing in a Bill, immediately after the meeting of Parliament, to extend the time fixed by the Act of last Session for carrying the provisions of the said Act into execution.

As Capt. Kater now confidently hopes that the models will be completed and deposited at the Exchequer in the course of the month of February, the Commissioners are of opinion, that if the period at which the new weights and measures are to be declared to be the only standards was postponed from the 1st of May 1825 (the day fixed by the Act of last Session), to the 1st of January 1826, sufficient time would be afforded for providing the models of the standard weights and measures required for the several counties and corporations of the United Kingdom, and for carrying into effect such of the enactments of the said Act as are preliminary to the general establishment of the new standards.

I have the honour to be,
Your obedient humble servant,
(Signed) George Clerk.

Having been requested to superintend the construction of the new models of weights and measures, (and very unexpected delays having taken place in their execution,) I beg to offer a short report of the progress which has been made, and of the impediments which have occurred.

On the 16th August, the making of the models of weight and capacity was confided to Mr. Bate, Mr. Troughton, in consequence of his advanced age, having declined the undertaking. Brass being a metal peculiarly liable to injury from the atmosphere of London, I directed Mr. Bate to make experiments on the best combination of tin and copper, which might serve as a substitute. These experiments occupied the remainder of the month of August.

In the beginning of September I left London, having previously given Mr. Bate ample and detailed instructions respecting every particular necessary for the construction of the models.

On my return, early in October, I learned from Mr. Bate that he had applied to Mr. Donkin the beginning of September, and that Mr. Donkin had then undertaken to turn the models for the bushel; but on the 5th of October, and not before, he informed Mr. Bate that he declined the execution of his engagement. Mr. Bate then proceeded to have models for the bushel cast by the best founders in London; but most unexpectedly

unexpectedly, out of twelve which were cast in various modes, only one proved sufficiently sound to be employed, the metal, on the removal of the exterior crust, appearing full of small holes, of various sizes. The attempt to conquer the difficulties of this part of the work occupied the remainder of October, the whole of November, and the greater part of December. In the mean time Mr. Bate proceeded with the other measures of capacity and with the weights; but as these presented no difficulties, his chief attention was directed to perfecting the bushel.

Two troy pounds were made, which I compared, on the 28th October, with the standard troy pound at Mr. Whittam's, in Abingdon-street. These weights were intended merely as the means of obtaining a near approximation to the avoirdupois pound, and to the weight of a gallon of distilled water.

On the 20th of December, Mr. Bate reported that he had six avoirdupois pounds ready, all the troy weights, and the subdivisions of the troy pound to grains.

It had been my intention to ascertain the capacity of the bushel by measurement, and I had employed myself in constructing the apparatus necessary for that purpose; but as it did not appear probable that the difficulties in casting the bushel would be speedily surmounted, I proposed, at a meeting of the Commissioners on the 21st of December, to determine the capacity of the bushel by the weight of distilled water it should contain, as this, under existing circumstances, would be the more accurate method, and would render unnecessary that nice attention to figure, which would otherwise be indispensably requisite.

All difficulty in the construction of the bushel being thus removed, Mr. Bate engaged to deliver to me, on the 1st of February, the following models, viz.—four bushels, four gallons, four quarts, four pints, four troy pounds, one avoirdupois pound, with sub-divisions to drams, a two-pounds, a four, a seven, a fourteen, a twenty-eight, and a fifty-six pounds avoirdupois; four weights, each equivalent to the weight of a gallon of distilled water, four to that of a quart of distilled water, and four to that of a pint. These models are intended to serve for constant use at the Exchequer, Guildhall, Edinburgh, and Dublin, the set of avoirdupois weights which will be ready by that time being for the Exchequer. Another set of models, superior in point of workmanship, though not in accuracy, will be afterwards made, and kept as standards to be transmitted to posterity.

I am in daily expectation of receiving from Mr. Bate a set of weights, for the purpose of enabling me to derive the avoirdupois

dupois from the troy pound, and thence the weight to be employed in determining the capacity of the gallon.

As no balance exists, either at the Mint or at the Bank of England, capable of weighing upwards of 280 pounds avoirdupois, I have given Mr. Bate the plan of a beam for this purpose, of great simplicity, and which, I trust, will be more accurate than any that has been hitherto made. This beam is also to be finished by the 1st of February.

The standards of linear measure have been prepared by Mr. Dollond, and are now ready for my final adjustment.

The Commissioners will perceive that no further difficulty exists; and should I receive the models from Mr. Bate by the 1st of February, according to his engagement, I trust I shall be able speedily to complete their adjustment, and that they will be ready for delivery in two or three weeks from that period.

York Gate, Regent's-park, HENRY KATER.
12th Jan. 1825.

To the Commissioners of Weights and Measures.
Whitehall Treasury Chambers, Feb. 4, 1825.

ON THE AGENCY OF PLATINA IN EFFECTING THE FORMATION OF WATER.

If in a tube closed at one end ammonia-muriate of platina is heated till completely decomposed, or if a solution of platina is treated with metallic zinc, part of the inside of this tube becomes covered with a thin coat of platina which adheres to it rather strongly. If afterwards a mixture of hydrogen and oxygen is made to pass under water, into this tube, the combination of the two gases is slowly effected at a moderate temperature. The phænomenon also takes place when these gases are put in contact with spongy platina moistened with water or with alcohol. If we try to substitute liquid ammonia or nitric acid for the water or alcohol, the gases no longer act upon each other. M. Dœbereiner thinks that this difference of effects is to be ascribed to the water or alcohol absorbing the gaseous mixture, and thus effecting immediate contact with the platina, which does not take place with the ammonia or nitric acid. By using vessels covered inside with platina, the hydrogen may be completely deprived of oxygen. However, it succeeds still better by putting it in contact, in very dry eudiometers, under mercury, with a porous ball formed of clay and of platina newly calcined. M. Dœbereiner describes a simple apparatus fit for determining with care the formation of water by means of platina.

At a little distance from the opening of a flask of the capacity

pacity of 4 to 5 cubic inches, a tube is soldered which at first is horizontal when the flask is upright, then descends vertically, and is terminated by a joint of copper fitted with a cock. This flask or this vial, (*phiole*) as the author calls it, is hermetically closed by a stopper whose axis is traversed by a platina wire terminating in a porous ball of clay and of platina sponge 2 to 4 lines in diameter. It is in this part of the apparatus that the combustion of the hydrogen takes place. A vacuum is at first produced, then the tin piece is screwed on a similar piece at the top of a graduated jar containing 60, 90, or 150 cubic inches, into which the mixture of two volumes of hydrogen and one of oxygen has been made to pass. The cock of the jar is first opened, not to be touched again till the end of the operation; then the upper cock, which is almost as soon shut: the part of the gaseous mixture which has passed into the flask enters directly into combustion; the water trickles along the sides, the platina becomes incandescent, and the vacuum is formed anew. The metal being sufficiently cooled, a new quantity of hydrogen and oxygen is introduced, and so on. To prevent all danger of the graduated jar being broken by the current of gas becoming inflamed, it is only necessary to place a ball of the same capacity as the flask between the two parts of the apparatus, and never to establish the communication excepting successively from one recipient to the other.—*Bulletin des Sciences*, No. 12.

Meteorological Observations at Great Yarmouth, by C. G. Harley, Esq.

[Continued from vol. lxiv. p. 317.]

	Days.		Winds.								Thermom.			Rain.
1824.	Dry.	Wet.	E.	SE.	S.	SW.	W.	NW.	N.	NE.	Low.	High.	Med.	In.
Sept.	10	20	—	4	6	6	2	3	3	6	47	73	64	$3\frac{1}{8}$
Oct.	9	22	—	5	9	5	5	5	0	2	41	65	54	5
Nov.	6	24	—	1	3	15	5	5	0	1	40	58	49	$3\frac{4}{8}$
Dec.	16	15	—	0	2	11	12	4	0	2	36	53	44	$3\frac{6}{8}$

The mean temperature of the last year is . . . . $53\frac{4}{12}$
The mean temperature for 30 years is . . . . . . $52\frac{10}{12}$
Quantity of water for the last year is . . . . $32\frac{1}{2}$
being the largest quantity for the last 24 years. The year 1812 approaches the nearest to it, the quantity being then . . . . $32\frac{3}{8}$
The mean quantity of water for 24 years is . . . $25\frac{1}{8}$

LIST

LIST OF NEW PATENTS.

To Francis Melville, of Argyle-street, Glasgow, piano-forte maker, for his improved method of securing the small piano-fortes commonly called "square piano-fortes" from the injuries to which they are liable from the tension of the strings.—Dated 18th January 1825.—6 months to enrol specification.

To Edward Lees, and George Harrison, brick-maker, of Little Thurrock, Essex, for an improved method of making bricks, tiles, &c.—1st February. —6 months.

To John Thin, of Edinburgh, architect, for a method of constructing a roasting-jack.—1st February.—2 months.

To Samuel Crosley, of Cottage Lane, City Road, Middlesex, for certain apparatus for measuring and registering the quantity of liquids passing from one place to another.—1st February.—6 months.

To Samuel Crosley, of Cottage Lane, City Road, Middlesex, for an improvement in the construction of gas regulators or governors.—1st Feb.—6 months.

To Timothy Burstall, of Bankside, Southwark, and John Hill, of Greenwich, engineers, for a locomotive or steam carriage.—3d Feb.—6 months.

To George Augustus Lamb, D.D., of Rye, Sussex, for a new composition of malt and hops.—10th February.—6 months.

To Richard Baduall junior, of Leek, Staffordshire, silk-manufacturer, for improvements in the winding, doubling, spinning, throwing, or twisting of silk, wool, cotton, &c.—10th February.—6 months.

To John Heathcoat, of Tiverton, Devonshire, lace-manufacturer, for improvements on the method of manufacturing silk.—11th February.—6 months.

To Edward Lees, of Little Thurrock, Essex, for improvements in waterworks, and in the mode of conveying water for the purpose of flooding and draining lands, applicable also to other useful purposes.—19th February.—6 months.

To Thomas Masterman, of the Dolphin Brewery, Broad-street, Ratcliffe, Middlesex, brewer, for an apparatus for bottling wine, beer and other liquids, with increased economy and dispatch.—19th February.—2 months.

To Edmund Lloyd, of North End, Fulham, Middlesex, for a new apparatus from which to feed fires with coals and other fuel.—19th February. —2 months.

To Benjamin Farrow, of Great Tower-street, London, ironmonger, for improvements in buildings, calculated to render them less likely to be destroyed or injured by fire than heretofore.—19th February.—6 months.

To Jesse Ross, of Leicester, hosier, for a new apparatus for combining and straightening wool, cotton, and other fibrous substances.—19th February. —6 months.

To Jacob Mould, of Lincoln's Inn Fields, Middlesex, for improvements in fire-arms:—communicated from abroad.—19th February.—6 months.

To Henry Burnett, of Arundel-street, Middlesex, for improvements in machinery for a new rotary or endless lever action:—communicated from abroad.—19th February.—6 months.

To John Beacham, of Paradise-street, Finsbury-square, cabinet-maker, for improvements in water-closets.—19th February.—2 months.

To James Ayton, of Trowse Millgate, Norfolk, miller, for an improvement or spring to be applied to bolting mills, for the purpose of facilitating and improving the dressing of flour and other substances.—19th February. —6 months.

Results

Results of a Meteorological Journal for the Year 1824, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

By William Burney, *LL.D.*

Latitude 50° 47′ 20″ North—Longitude 1° 7′ West of Greenwich. In time 4′ 28″.

Months.	Barometer. Max.	Min.	Media.	Range.	No. of Changes.	Spaces described.	Greatest Variation in 24 hours.	Media at 8 A.M.	Media at 2 P.M.	Media at 8 P.M.	Self-registering Thermometer. Max.	Min.	Media.	Mean Range.	Gt. Var. in 24 hours	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.	Mean Temp. of Spring Water.	De Luc's Hygrometer. Max.	Min.	Mean Range of the Index.	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.	Media at 8,2,&8.o'cl.
	In.	In.	In.	In.		In.	In.	In.	In.	In.	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	7 °
January	30·54	28·84	30·055	1·70	18	7·22	0·98	30·056	30·043	30·058	52	25	39·93	27	16	43·77	37·87	39·81	49·65	93	56	37	70·7	76·2	74·4	73·8
February	30·47	28·77	29·786	1·70	18	5·70	0·83	29·787	29·785	29·786	52	31	42·39	21	14	45·79	40·62	42·38	49·04	86	54	32	67·9	75·0	74·6	62·5
March ...	30·32	29·02	29·810	1·30	31	8·85	0·88	29·798	29·801	29·828	57	29	43·53	28	20	47·81	41·42	41·81	48·62	81	39	42	59·7	69·4	67·7	65·6
April ...	30·44	29·19	29·876	1·25	20	9·98	0·93	29·869	29·861	29·893	63	31	47·53	32	18	51·93	46·23	46·67	48·06	83	40	43	54·4	63·7	62·7	50·3
May ...	30·64	29·46	29·959	1·18	20	5·28	0·45	29·959	29·962	29·957	73	38	54·21	35	22	60·13	53·65	52·48	48·43	90	33	57	50·4	59·2	63·0	57·5
June ...	30·36	29·20	29·897	1·16	21	5·82	0·59	29·896	29·897	29·901	76	47	59·60	29	24	66·16	59·37	57·27	49·47	82	38	44	51·0	55·7	62·4	56·4
July ...	30·50	29·62	30·038	0·88	30	4·94	0·42	30·042	30·047	30·029	78	50	63·98	28	23	70·74	64·77	62·45	50·82	88	38	50	48·6	54·2	59·3	54·0
August	30·37	29·60	29·961	0·77	23	4·86	0·45	29·957	29·964	29·961	77	46	62·94	31	24	68·84	62·87	61·19	52·00	92	47	45	55·3	62·2	66·1	61·2
Septemb.	30·27	29·40	29·899	0·87	21	4·82	0·54	29·897	29·904	29·900	79	38	60·62	41	22	66·03	60·03	59·03	53·09	84	50	34	57·3	66·4	69·6	64·4
October	30·18	28·78	29·644	1·40	18	6·87	0·69	29·629	29·634	29·658	68	32	53·35	36	23	57·51	52·68	52·42	53·42	88	48	40	64·3	70·7	71·5	68·8
Novemb.	30·19	28·47	29·663	1·72	28	8·67	0·93	29·665	29·663	29.663	60	34	50·22	26	20	53·17	49·03	50·33	52·91	81	52	29	66·3	71·4	70·3	69·3
Decemb.	30·44	28·95	29·842	1·49	27	10·11	0·95	29·819	29·837	29·869	55	30	45·82	25	20	48·90	44·77	45·52	52·04	90	58	32	70·3	75·8	76·1	74·1
Averages for 1824.	30·64	28·47	29·869	15·42	275	83·12	0·98	29·864	29·866	29·875	79	25	52·01	29·9	24	56·73	51·11	50·94	50·63	93	33	40·4	59·7	66·6	68·1	64·8

Months. 1824.	Scale of the Winds.									Modifications of Clouds.							Weather.						Atmospheric Phænomena.									Evaporation in Inches, &c.	Rain in Inches, &c.
	North.	North-East.	East.	South-East.	South.	South-West.	West.	North-West.	Total Number of Days.	Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostratus.	Nimbus.	A clear Sky.	Fair, with Clouds.	An overcast Sky.	Foggy.	Rain, &c.	Total Number of Days.	Anthelion.	Parhelia.	Paraselenæ.	Solar Halos.	Lunar Halos.	Rainbows.	Meteors.	Lightning.	Thunder.		
January ..	9	1	2	1	1	7	1½	8	31	21	7	30	0	7	10	13	2	14	9	1	4	31	0	1	0	2	1	0	16	0	0	0·77	0·990
February	1	6	3½	6	2	6½	2½	1½	29	14	2	28	0	9	14	19	2	9	12	1	6	29	0	0	0	1	1	1	3	0	0	0·98	2·685
March ...	6	2½	0	2	1	8	1½	10	31	16	10	29	0	17	20	23	3	10	10	1	7	31	0	2	0	1	2	2	2	0	0	1·67	3·775
April	4½	5½	2	5	1	5	2	5	30	19	9	25	0	15	11	17	3	12	9	0	6	30	0	3	0	5	1	2	9	1	1	2·34	2·522
May	4½	6	4	5	1	1	3½	6	31	13	10	23	1	19	19	22	2	11	11	0	7	31	0	2	3	4	4	1	2	0	0	3·22	4·590
June .. .	1½	10	1	6	1½	5½	3½	1	30	18	7	21	1	20	22	16	5	11	8	0	6	30	0	4	0	2	0	0	4	0	0	4·49	2·915
July	3	3	4½	3	2	7	4½	4	31	27	20	30	1	24	13	12	2	20	5	0	4	31	0	5	0	2	0	0	12	0	0	5·58	2·910
August ..	4	2	2	2	3	8	6	3½	31	22	21	29	3	26	20	19	4	15	5½	½	6	31	0	0	0	0	1	0	24	2	0	4·50	3·450
September	2	1½	1½	3½	5½	6½	2½	7	30	17	13	27	4	19	17	21	3	14	5½	1	6½	30	1	1	0	0	1	4	11	1	1	3·40	3·830
October ..	3	½	4	3	2½	7¼	4½	6	31	22	18	28	8	23	21	25	2	12	8	0	9	31	0	0	3	2	2	7	10	2	0	2·70	3·520
November	½	2	1	1	2½	9½	8	5½	30	15	9	28	1	12	20	24	1	8	11	0	10	30	0	1	1	2	2	2	3	1	1	2·10	5·305
December	2	1	0	¼	¼	9	10½	7½	31	21	7	26	7	11	7	21	2	11	8	1	9	31	0	1	0	2	1	1	4	1	1	1·00	3·565
Averages for 1824.	41	41½	25½	37¾	23¾	81	50½	65	366	225	133	324	26	202	194	232	31	147	102	5½	80½	366	1	20	7	23	16	20	100	8	4	32·75	40·057

ANNUAL RESULTS FOR 1824.

Barometer.	Inches.
Greatest pressure of atmosphere, May 27, Wind N.W.	30·640
Least ditto ditto Nov. 23d, Wind S.S.W.	28·470
Range of the mercury	2·170
Annual mean pressure of the atmosphere	29·869
Mean pressure for 173 days, with the moon in North declination	29·855
Mean pressure for 183 days, with the moon in South declination	29·837
Annual mean pressure at 8 o'clock A.M.	29·864
——————— at 2 o'clock P.M.	29·866
——————— at 8 o'clock P.M.	29·875
Greatest range of the mercury in November . . .	1·720
Least range of ditto in August	0·770
Greatest annual variation in 24 hours in January .	0·980
Least of the greatest variations in 24 hours in July .	0·420
Aggregate of the spaces described by the rising and falling of the mercury	83·120
Number of changes	275·

Self-registering Day and Night Thermometer.

Greatest thermometrical heat, Sept. 3d, Wind W. .	79°
——————— cold, Jan. on 3 different nights	25
Range of the thermometer between the extremes .	54
Annual mean temperature of the external air . .	52·01
——————— of do. at 8 A.M. . . .	51·11
——————— of do. at 8 P.M. . . .	50·94
——————— of do. at 2 P.M. . . .	56·73
Greatest range in September	41·00
Least of the monthly ranges in February . . .	21·00
Annual mean range	29·90
Greatest monthly variation in 24 hours in June and August	24·00
Least of the greatest variations in 24 hours in Feb.	14·00
Annual mean temperature of spring water at 8 A.M.	50·68

De Luc's *Whalebone Hygrometer.*	Degrees.
Greatest humidity of the atmosphere on the 22d Jan.	93
Greatest dryness of ditto on the 23d May	33
Range of the index between the extremes	60
Annual mean of the hygrometer at 8 o'clock A.M.	66·6
——————— at 8 o'clock P.M.	68·1
——————— at 2 o'clock P.M.	59·7
——————— at 8, 2, & 8 o'clock	64·8

Greatest mean monthly humidity of the atmosphere in December	Deg. 74·1
Greatest mean monthly dryness of the atmosphere in July	54·0

Position of the Winds.

	Days.
From North to North-east	41
—— North-east to East	$41\frac{1}{2}$
—— East to South-east	$25\frac{1}{2}$
—— South-east to South	$37\frac{3}{4}$
—— South to South-west	$23\frac{3}{4}$
—— South-west to West	81
—— West to North-west	$50\frac{1}{2}$
—— North-west to North	65
	——366

Clouds, agreeably to the Nomenclature; or the number of days on which each modification has appeared.

	Days.
Cirrus	225
Cirrocumulus	133
Cirrostratus	324
Stratus	26
Cumulus	202
Cumulostratus	194
Nimbus	232

General State of the Weather.

	Days.
A transparent atmosphere without clouds .	31
Fair, with various modifications of clouds .	147
An overcast sky, without rain	102
Foggy	$5\frac{1}{2}$
Rain, hail, snow, and sleet	$80\frac{1}{2}$
	——366

Atmospheric Phænomena.

	No.
Anthelion, or mock-sun diametrically opposite to the true sun	1
Parhelia, or mock-suns on the sides of the true sun	20
Paraselenæ, or mock-moons	7
Solar halos	23
Lunar halos	16
Rainbows, solar and lunar	20
Meteors of various sizes	100
Lightning, days on which it happened . .	8
Thunder, ditto ditto	4

Evaporation.

	Inches.
Greatest monthly quantity in July	5·58

Least

Least monthly quantity in January	0·77 In.
Total amount for the year	32·75

Rain.

Greatest monthly depth in November . .	5·305
Least monthly depth in January	0·990
Total depth for the year near the ground .	40·057
Total depth for the year 23 feet high . .	35·549

N. B. The barometer is hung up in the observatory 50 feet above the low-water mark of Portsmouth Harbour; and the self-registering horizontal day and night thermometer, and De Luc's whalebone hygrometer, are placed in open-worked cases, in a northern aspect, out of the rays of the sun, 10 feet above the garden ground. The pluviameter and evaporator have respectively the same square area: the former is emptied every morning at 8 o'clock after rain, into a cylindrical glass gauge accurately graduated to $\frac{1}{100}$th of an inch; and the quantity lost by evaporation from the latter is ascertained at least every third day, and sometimes oftener, when great evaporations happen by means of a high temperature and dry northerly or easterly winds.

Barometrical Pressure.—The *maximum* pressure was higher this year by $\frac{1}{25}$th of an inch than it was in 1823, and the *minimum* pressure was less by $\frac{1}{20}$th of an inch. The mean pressure this year is $\frac{19}{100}$ths of an inch less than that of last year, but it agrees with the mean pressure for the last 10 years within $\frac{3}{1000}$ths of an inch. The aggregate of the spaces described by the alternate rising and falling of the mercury is 3¼ inches greater than that of last year; and the number of changes is 21 more.—For 173 days in which the moon ranged in North declination, the mean pressure was $\frac{9}{100}$ths of an inch greater than that in the 183 days in which she ranged in South declination.

The mean barometrical pressures for the last *six years*, while the moon was in North and South declination, are as follow:

With the moon in North declination .	29·885 inches.
With the moon in South declination .	29·845 inches.
Increased pressure for her position North of the Equinoctial	·04

Here I must observe that this difference of $\frac{1}{25}$th of an inch in the elevation of the barometer, by the superior weight of the atmosphere while the moon was in *north declination*, is not sufficient, in a local point of view, to produce any sensible effects over the atmosphere in respect to the weather in this latitude; and that the difference in the course of a longer series of years might, perhaps, become inconsiderable, and thus

thus annul the idea of the existence of a superior pressure by the Moon's influence in either hemisphere. I am supported in this opinion from similar observations having been made by Luke Howard, Esq. F.R.S., in the years 1807 and 1816; as in both these years the mercury in his barometer is said to have been nearly as high again as the above resulting difference, while the moon was in *south declination.*—At some future time more may be said on this subject.

Temperature.—The mean temperature of the external air a few feet from the ground this year is $1\frac{47}{100}$ of a degree more than that of 1823, and is $\frac{4}{23}$ths of a degree higher than the mean temperature for the last eight years.—Although July was the hottest month, yet the *maximum* temperature did not take place till the 3rd of September: it has not occurred so late in the summer as this since the year 1815, and very generally takes place in June, when the sun is nearly at his greatest north declination, before or after entering the sign Cancer.

From the great quantity of rain that has fallen this year, and the abundant floating vapours, the strength of the sun's rays on the surface of the earth seems to have been diminished; for the mean temperature of spring water has fallen short of its yearly average, and for the last four years at 8 A.M. is $\frac{17}{23}$ths of a degree less than the mean temperature of the air for the same period.

The mean state of the hygrometer this and the preceding year coincides within three-tenths of a degree; and the means of the observations thereon at 8 o'clock A.M., in both years, exactly agree.

Wind.—In comparing the scales of the prevailing winds in 1823 and 1824, there appears to be a near accordance, except in the North and North-east winds, which have blown comparatively longer from these points of the compass.

The following is the number of strong gales of wind, or days on which they have prevailed this year:

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
7	14	4	5	9	45	6	10	100

The gales from the south-west point, as usual, are nearly half the number in the scale.

Clouds.—The following is a correct scale of the clouds agreeably to the nomenclature, being the number of days on which each modification has appeared during the last *eight* years, ending with 1824. Cirrus.

Cirrus.	Cirro-cumulus.	Cirro-stratus.	Stratus.	Cumulus.	Cumulo-stratus.	Nimbus.
1622	1334	2260	276	1498	1461	1681

By these curious results, we find that the *cumulus* and *cumulostratus* approximate nearest in number: the former is a fair-weather cloud, and evaporates at or soon after sunset when the atmosphere is not in a humid state; the latter is generally a prognostic of an approaching change in the state of the atmosphere. Next to these, the *cirrus* and *nimbus* approximate nearest in number: the *cirri* are precursors of, and very generally become the crowns of, the passing *nimbi.* The respective electricities they at all times possess, are *positive* and *negative*, and the rain is induced by their inosculation, gravity, and electric *effluvia*. Of all the modifications of clouds, the *cirrostratus*, it will be perceived, prevails most, being frequently formed from the descending *cirrus*, and sometimes from the *cumulus*, when changes are about to take place in the direction of the winds, and in the temperature of the atmosphere. The proportional appearance of the *cirrostratus* to the *cirrus* is as 113 to 81. The *cirrocumulus* and *stratus* are also fair-weather clouds; the former is an indication of increasing heat, and is generally transformed into *cirrostratus* with a moist wind; and the latter into nascent *cumulus*, after sunrise.

Weather.—The year, although not cold, was generally wet and windy, particularly the last four months, during which time many of the vales and low parts of England were often under water, which occasioned the loss of both lives and property to a considerable amount. There has been rain, more or less, on 232 days; but 80 days and nights is the time it has rained.

The hurricane that blew across this country on the 22d and 23d of November last will be long remembered, from the great loss in wrecks along the southern shores. It was felt powerfully in the Western Ocean at the same time.

Nothing peculiar has occurred this year in the appearance of atmospheric and meteoric phænomena.

About six minutes before 2 o'clock in the afternoon of the 6th of December, a shock of an earthquake was very generally felt in these towns and neighbourhood; also at Havant, Emsworth, Chichester, Bognor, and Arundel, that is, in the direction of from S.W. to N.E. It was accompanied with a rumbling noise, and put both light and heavy furniture in a tremour about five or six seconds of time, It is now about twelve years since the last shock was felt here, which occurred in the night, and was more violent than this one.

METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr. Burney at Gosport, Mr. Cary in London, and Mr. Veall at Boston.*

Days of Month, 1825.	Gosport, at half-past Eight o'Clock, A.M. Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Clouds. Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Height of Barometer, in Inches, &c. Lond. 1 P.M.	Bost. 8½ A.M.	Thermometer. London. 8 A.M.	Noon.	11 P.M.	Boston. 8½ A.M.	Rain. London.	Boston.	Weather. London.	Boston.	Wind.
Jan. 26	30·01	39	50·10	69	S.	...	......	...	...	1	...	...	...	1	30·02	29·85	32	38	40	29·5	...	...	Cloudy	Fine, rain and snow	Calm
☽ 27	30·05	48	......	84	W.	...	......	...	...	1	...	...	1	1	30·09	29·80	40	50	45	44·0	...	·09	Cloudy	Cloudy [p.m.	S.
28	30·54	35	......	77	NW.	0·05	......	...	...	1	...	...	1	...	30·72	30·35	34	44	40	35·0	...	...	Fair	Fine	W.
29	30·68	33	......	78	NW.	...	......	...	...	1	...	1	...	...	30·72	30·55	32	42	34	29·0	...	...	Fair	Fine	Calm
30	30·51	41	......	76	SW.	...	0·010	...	...	1	...	...	1	1	30·45	30·32	38	46	45	39·0	...	...	Cloudy	Cloudy	W.
31	30·50	47	50·00	92	W.	·06	·020	...	...	1	...	...	1	1	30·52	30·30	45	50	46	44·5	...	...	Cloudy	Cloudy	Calm
Feb. 1	30·24	47	......	80	SW.	...	·070	...	...	1	...	...	1	1	30·10	29·75	46	46	39	47·0	...	...	Rain	Cloudy	S.
2	30·37	34	......	76	NW.	...	·010	...	...	1	...	...	...	1	30·28	30·15	32	43	46	35·0	0·48	...	Cloudy	Fine, rain at night	W.
○ 3	29·55	50	......	76	SW.	·10	·040	1	1	1	...	...	1	1	29·50	29·18	47	47	34	45·0	...	·06	Stormy	Stormy	W.
4	29·62	35	......	67	NW.	...	......	...	...	1	...	1	1	1	29·72	29·40	32	35	30	27·5	...	...	Snow	Fine, snow a.m	W.
5	29·66	31	......	67	NW.	...	·010	1	...	1	...	1	1	1	29·72	29·50	27	35	32	30	...	...	Fair	Fine	W.
6	29·94	32	......	68	NW.	·10	......	...	...	1	...	1	...	...	30·14	29·90	33	40	33	32	...	...	Fair	Cloudy	W.
7	30·12	40	49·90	72	SW.	...	·335	...	...	1	...	...	...	1	30·14	30	35	43	40	32·5	...	...	Cloudy	Cloudy, rain at night	S.
8	29·94	41	......	80	NW.	...	......	...	...	1	...	1	1	...	30·02	29·75	40	45	36	34·5	...	·11	Fair	Fine	W.
9	30·27	35	......	77	NW.	·07	......	1	...	...	...	1	...	...	30·40	30·10	35	45	40	35·5	0·12	...	Fair	Fine	W.
☾ 10	30·43	43	......	75	W.	...	......	1	1	1	...	1	...	...	30·52	30·15	40	47	37	42	...	...	Fair	Cloudy	S.
11	30·50	39	......	77	E.	...	......	1	...	1	...	1	...	...	30·58	30·33	36	47	37	34	...	...	Fair	Fine	Calm
12	30·48	37	......	80	N.	·08	......	1	...	1	...	1	...	...	30·58	30·35	35	45	35	38	...	...	Fair	Cloudy	Calm
13	30·50	40	49·75	73	NW.	...	......	...	...	1	...	...	...	...	30·58	30·35	32	37	38	41·5	...	...	Foggy	Cloudy	Calm
14	30·35	41	......	76	E.	...	......	...	...	1	...	...	...	...	30·42	30·20	38	38	37	41	...	...	Cloudy	Cloudy	Calm
15	30·07	44	......	77	SE.	·04	......	...	...	1	...	...	...	1	30·13	30·03	37	40	39	36	...	...	Cloudy	Cloudy	Calm
16	29·92	45	......	77	S.	...	......	1	1	1	...	1	1	1	30·04	29·80	39	45	44	41	0·00	...	Cloudy	Cloudy	Calm
17	29·97	46	......	75	S.	...	·250	...	...	1	...	...	1	1	30·05	29·80	45	47	45	40	...	...	Cloudy	Cloudy, rain at night	S.
18	29·88	47	......	81	E.	·05	·080	...	...	1	...	...	1	1	30·06	29·80	45	45	45	43	...	·34	Rain	Cloudy	E.
19	30·15	43	49·75	81	NE.	...	......	...	1	1	...	...	1	...	30·25	30·05	45	49	46	44	...	...	Cloudy	Cloudy	E.
20	30·23	48	......	69	SW.	...	......	1	1	1	...	1	1	1	30·35	30	47	51	42	43·5	...	...	Fair	Cloudy	SE.
21	30·40	46	......	76	W.	·15	......	...	...	1	...	...	1	...	30·50	30·25	37	43	39	34·5	...	...	Foggy	Fine	W.
22	30·39	45	......	68	E.	...	......	1	...	1	...	1	1	...	30·42	30·30	38	44	36	33·5	...	...	Fair	Fine	W.
23	30·10	42	......	69	E.	...	......	1	...	1	...	...	1	...	30·30	30·08	36	38	37	39	0·20	...	Cloudy	Cloudy	E.
24	30·13	37	......	77	NE.	...	......	...	...	1	...	...	1	...	30·35	30·10	35	40	37	38	...	...	Cloudy	Cloudy	E.
25	30·31	39	49·40	63	NE.	·30	......	...	...	...	...	...	1	...	30·40	30·30	37	40	35	38	...	...	Cloudy	Cloudy	Calm
Aver.:	30·187	40·97	49·82	75·2		1·00	0·825	10	5	29	0	12	19	15	30·26	30·02	38	43	39	37·6	0·80	·60			

THE

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XXV. *On the Method of the Least Squares.* By J. Ivory, *Esq. M.A. F.R.S.*

[Concluded from p. 88.]

4. ADMITTING the expression $\phi(e^2)$ for the probability of an error, we may both demonstrate the rule of the least squares, and determine the form of the function, by the following short process:

Let P stand for the product,

$$\phi(e^2).\phi(e'^2).\phi(e''^2) \text{ \&c.};$$

then the most probable system of the errors will be found by the equations which make P a maximum, viz.

$$\frac{dP}{dx} = 0, \quad \frac{dP}{dy} = 0, \text{ \&c.}$$

Now, n being the number of the errors, we have

$$P = k^n c^{\log.\frac{P}{k^n}}:$$

but $$\log.\frac{P}{k^n} = \log.\frac{\phi(e^2)}{k} + \log.\frac{\phi(e'^2)}{k} + \text{\&c.}$$

Again, by expanding $\phi(e^2)$, we get

$$\frac{\phi(e^2)}{k} = 1 - h^2e^2 - ge^4 - \text{\&c.};$$

wherefore, $$\log.\frac{\phi(e^2)}{k} = -h^2e^2 - \left(g - \frac{h^4}{2}\right)e^4 - \text{\&c.}$$

Hence $$\log.\frac{P}{k^n} = -h^2 S.e^2 - \left(g - \frac{h^4}{2}\right)S.e^4 - \text{\&c.}:$$

and, by substituting this value in the foregoing differential equations, we get,

$$o = h^2.\frac{d.S.e^2}{dx} + \left(g - \frac{h^4}{2}\right)\frac{d.S.e^4}{dx} + \text{\&c.},$$

$$o = h^2.\frac{d.S.e^2}{dy} + \left(g - \frac{h^4}{2}\right)\frac{d.S.e^4}{dy} + \text{\&c.}$$

But as we are not now in search of a mere mathematical

 Theory,

Theory, we must choose in the general expressions those particular cases only that are capable of being applied practically, and that are not too complicated for real use. The calculations would become impracticable if the equations to be solved passed the first degree. If, therefore, we only retain the parts of the foregoing equations which are of one dimension with respect to the errors, we shall get

$$\frac{d.\mathrm{S}.e^2}{dx}=0,\ \frac{d.\mathrm{S}.e^2}{dy}=0,\ \&c.,$$

and this proves that the value of $\mathrm{S}.e^2$ must be a minimum. The same simplification leads to the equation, $\log.\frac{\varphi(e^2)}{k} = -h^2e^2$; whence

$$\varphi(e^2)=k\,c^{-h^2e^2}.$$

The foregoing investigations are at least clear and simple. It follows as an unavoidable consequence, that if we adopt the rule of the least squares as the most advantageous determination of a system of errors, the law of probability can be nothing else but the function $kc^{-h^2e^2}$. On the other hand, when we apply the doctrine of probabilities to find the most advantageous method of combining a set of errors, we shall fall upon the method of the least squares, if the chance of an error be expressed by the function $kc^{-h^2e^2}$; but if the law of the errors be different, the same rule will no longer be true. The two things are necessarily connected, in so much that the one leads exclusively to the other. The method of the least squares cannot possibly be true in any other law of probability than the one we have mentioned.

Now the conclusion which has just been stated, is directly at variance with what Laplace has determined in the *Theorie Analytique des Probabilités*, liv. 2. cap. 4. § 20. In a system consisting of a great number of observations it is proved, in the work we have cited, that the rule of the least squares ought to be employed whatever be the law of the chance of an error (p. 321, 3d edition). It is the generality of the conclusion that chiefly constitutes the merit of this demonstration. It must be added, that M. Poisson, who has lately treated the same subject in the *Connaissance des Tems* 1827, has arrived at the same conclusions with Laplace, which are thus confirmed. All other authors likewise acquiesce in the result of Laplace's investigation, and admit that the rule of the least squares may subsist with different laws of probability. Every authority is thus directly opposed to the opinion I have ventured to express. The words of the poet,

"Nullius addictus jurare in verba magistri,"

contain

contain a maxim which ought to have greater force in the mathematics than in any other branch of learning. But it might not be found altogether devoid of truth, if we were to affirm that, in the present times, the voice of authority has a more decisive influence in that science than in any other. It is so much easier to approve or disapprove on the credit of a few great names, than it is to find the skill and the patience requisite to examine a knotty point of abstract science. Having always exercised my own judgement in such speculations, I shall claim the same privilege in the present instance; and I doubt not to be able to prove in a satisfactory manner, that Laplace's demonstration is not general, as it is stated to be, but is really confined to the particular law already mentioned.

It will be sufficient for the purpose I have in view to consider the simplest case of Laplace's investigation, that for finding one element by means of a set of equations of condition: liv. 2. cap. 4, No. 20. It is assumed that the errors are determined by the equation $S.\lambda e = 0$, that is,

$$\lambda e + \lambda' e' + \lambda'' e'' + \&c. = 0,$$

where λ, λ', λ''* &c. are integer numbers bearing any proportions to one another; and the scope of the investigation is to determine these factors so as to obtain the most advantageous, or the most probable, system. The author goes back to the first principles of the doctrine of probabilities, or to the theory of combinations; and he investigates the chance that the function $S.\lambda e$ shall have a given value l. If we adopt the notation used here, and write h^2 for $\frac{k}{4k''.a^2}$, the expression of the probability sought, found in p. 317, 3d edition, is this,

$$\frac{h}{\sqrt{\pi S.\lambda^2}}\, c^{-h^2 . \frac{l^2}{S.\lambda^2}}.$$

Again, if we substitute the values of the errors given by the equations of condition in the equation $S.\lambda e = 0$, we get the value of the element x equal to $\frac{S.\lambda m}{S.\lambda a}$; and if we substitute $\frac{S.\lambda m}{S.\lambda a} + u$ for x, in the equation $S.\lambda e = l$, we shall get

$$l = u \times S.\lambda a.$$

The foregoing expression therefore becomes

$$\frac{h}{\sqrt{\pi S.\lambda^2}}\, c^{-h^2 \frac{(S.\lambda a)^2}{S.\lambda^2} . u^2};$$

* As the letter m, which Laplace uses for the coefficients of the errors, is pre-occupied in the equations of condition, it became necessary to introduce another letter here.

and

and it is proportional to the probability of u. To find the fraction equal to the absolute chance, we must divide the foregoing expression by the sum of all the probabilities for every possible value of u, that is, by the value of the integral

$$\frac{h}{\sqrt{\pi S.\lambda^2}} . \int d u \, c^{-h^2 \frac{(S.\lambda a)^2}{S \lambda^2} . u^2},$$

taken between the limits $\pm \infty$. The integral is equal to $\frac{1}{S.\lambda a}$; and hence the absolute probability of u is equal to

$$\frac{h.S.\lambda a}{\sqrt{\pi S.\lambda^2}} c^{-h^2 \frac{(S.\lambda a)^2}{S.\lambda^2} u^2}.$$

Now if we put $u = 0$, the value of the above expression is $\frac{h}{\sqrt{\pi}} \times \frac{S.\lambda a}{\sqrt{S.\lambda^2}}$, which is therefore the probability that the element x is equal to $\frac{S.\lambda n}{S.\lambda^2}$, or that the errors of observation coincide with the system $S.\lambda e = 0$. The factors λ, λ', λ'' &c. must next be determined so as to make the probability a maximum, or so as to make $\frac{\sqrt{S.\lambda^2}}{S.\lambda a}$ a minimum, which is the conclusion Laplace has arrived at, p. 319, by considering the matter a little differently. It is found that the maximum will take place when

$$\frac{\lambda}{a} = \frac{\lambda'}{a'} = \frac{\lambda''}{a''} \text{ \&c.};$$

and consequently the most probable system of errors is found by the equation $S.ae = 0$, which is the condition that makes $S.e^2$ a minimum. It is to be observed too, that no step of the investigation depends upon the value of h, or upon the law of probability, which is left indeterminate.

The foregoing reasoning is detailed particularly in order to prove clearly, what might have been inferred from the analysis of Laplace, that the system of errors $S.ae = 0$, is more probable than any other system $S.\lambda e = 0$, in which λ, λ', λ'' &c. are not all in the same proportion to a, a', a'' &c. Now this being proved, I say, that the law of the probability of an error is thereby fixed, and no longer remains indeterminate, as stated by Laplace. Let $\varphi(e^2)$ denote the probability of the error e: then the probability of any system, or the probability of the simultaneous existence of the errors e, e', e'' &c. of that system, is equal to

$$\frac{1}{H} \times \varphi(e^2).\varphi(e'^2).\varphi(e''^2) \text{ \&c.} = \frac{P}{H},$$

the letter H representing the sum of all the values of P for every

every possible system. It has been shown that the product P has a maximum value determined by the equation

$$\left(\frac{a}{\varphi(e^2)} \cdot \frac{d.\varphi(e^2)}{d e^2}\right) e + \left(\frac{a'}{\varphi(e'^2)} \cdot \frac{d.\varphi(e'^2)}{d e'^2}\right) e' + \&c. = 0 \quad (B)$$

Now it is plain that (B) will coincide either with some system $S.\lambda e = 0$, the numbers λ, λ', λ'' &c. not being all in the same proportion to a, a', a'' &c.; or it will coincide with the particular system $S.ae = 0$. The former will be the case when $\frac{1}{\varphi(e^2)} \cdot \frac{d.\varphi(e^2)}{d e^2}$ contains e, and consequently has different values for the several errors; the latter will be the case when the same function is equal to a constant quantity. The equation (B), in which are contained all the most probable systems, cannot coincide with any case $S.\lambda e = 0$; for then, according to the demonstration of Laplace, the probability would be less than in the system $S.ae = 0$, which is absurd. Therefore the only supposition that can possibly be true is the identity of (B) with $S.ae = 0$. But this requires that

$$\frac{1}{\varphi(e^2)} \cdot \frac{d.\varphi(e^2)}{d e^2} = -h^2;$$

and consequently, $\varphi(e^2) = k c^{-h^2 e^2}$.

Since the equation $S.ae = 0$, makes the function $S.e^2$ a minimum, it follows incontestably, from the demonstration of Laplace, that the minimum of $S.e^2$ must coincide with the maximum of the product P; which is the same condition to which we have already brought the determination of the probability of an error by a different mode of reasoning. Therefore the investigation of Laplace, whatever merit it may have in other respects, is neither more nor less general than the other solutions of the problem.

The analysis of Laplace is different from that of other mathematicians in reversing the order of investigation. It has been most usual to begin with seeking the law of the probability of an error; and, when this is found, the chance of a given combination of the errors is derived from it. Laplace begins with computing the chance of a given combination of the errors by means of the doctrine of combinations; but it is manifest that the result obtained, when compared with the equations supposed to subsist between the errors, leads to a particular law of probability. Upon the premises laid down, both methods lead to the same conclusions, and the demonstrations obtained in both ways are equally extensive. The investigation of Laplace is more analytical and more philosophical, as it requires no previous discussion of the law of the probability of an error; but, on the other hand, it is confined

to

to the case of a great number of errors, in order to render the calculations practicable.

Thus, if we apply the doctrine of probabilities to find the most advantageous way of combining a set of observations, and likewise require that the final equation must be one of the first degree, we are invariably led to the method of the least squares, and to the law of probability expressed by the function $kc^{-h'e^2}$, in whatever way we pursue the investigation. It must therefore be allowed that the evidence we have for one of these two things is just the same as that which can be obtained for the other. If it can be well proved that the particular law of probability will belong to every set of observations, the rule of the least squares will be firmly established; but if hardly any good reason can be alleged in support of the first, the other will rest on foundations equally feeble. When the investigation of Laplace is understood in all the generality that, I apprehend, has hitherto been ascribed to it, the proof of the method of the least squares is as strong and convincing as the nature of the case will admit; because among all the laws of probability that can be imagined, there must be one that will nearly apply to the errors of any set of observations in which only an ordinary degree of regularity is supposed to prevail. But the complexion of the proof is entirely changed when it is shown that the author's reasoning takes in only one particular law.

What has now been said justifies the view taken of this theory in the foregoing researches. The proof of the method of the least squares by means of the doctrine of probabilities, being entirely supposititious and mathematical, is insufficient and unsatisfactory; and we must therefore seek a better support for it in the nature of the equations of condition. I have already given two different demonstrations independent of the laws of chance. On re-considering these I do not find that any thing material can be added to the second; but some considerations that have occurred since the first was written seem to render it more complete.

If, as in the first demonstration, we conceive the weights a, a', a'' &c. to be *in equilibrio* when suspended from the levers e, e', e'' &c., it is obvious that the equilibrium will not be disturbed, if all the errors increase or decrease in the same proportion. Now, when the errors vary in this manner, there can be no manner of doubt that the most advantageous system is that which makes the sum of their squares a minimum. Suppose next that the errors vary, but not all in the same proportion; the equilibrium will be destroyed, and the force of preponderancy

preponderancy may be estimated by the distance, from the common fulcrum, of the centre of gravity of the weights hanging from their new points of suspension. But whatever the supposed variations are, it will be admitted that the errors may undergo opposite variations, so as to acquire an equal and contrary preponderancy; and it is obvious that the system of the least squares is an exact mean between the two opposite systems. We cannot therefore but conclude that the system in which the sum of the squares is a minimum, which occupies the mean place among all the possible systems, is preferable to every other.

When the rule of the least squares is demonstrated in a satisfactory manner from the nature of the equations of condition, it has been shown that the errors can follow only one law of probability. But it would be in vain to attempt to verify this law in a direct manner, or to show that the errors of any set of observations exactly agreed with it, or even approximated to it. A very cautious inquirer might therefore wish to compare the results obtained by the doctrine of probabilities with the like results deduced immediately from the equations of condition by the ordinary processes of investigation. There is no doubt that a very exact coincidence would be found between the conclusions deduced from the two methods; but we cannot enter upon this discussion.

The practice which is universally followed of taking the arithmetical mean of a set of observations is comprehended in the general method we have been considering, as we have always supposed; for it is the particular case when the weights a, a', a'' &c. are all equal, and the sum of the errors is equal to zero. It may not, however, be improper to prove this more particularly; and for this purpose we must go back to the original meaning of the symbols. We have

$$e = \mathrm{V} - o$$
$$\mathrm{V} = \mathrm{V}' + \frac{d\mathrm{V}'}{dx}x = \mathrm{V}' + ax:$$

now when a, or $\frac{d\mathrm{V}'}{dx}$, remains invariable from one observation to another, the value of V will likewise be constantly the same. Wherefore the errors are respectively

$$\begin{aligned} e &= \mathrm{V} - o \\ e' &= \mathrm{V} - o' \\ e'' &= \mathrm{V} - o'' \\ &\text{\&c.} \end{aligned}$$

and when the sum of the errors is zero, their number being n, we obtain $\mathrm{V} = \frac{o + o' + o'' + \text{\&c.}}{n}$.

In

In conclusion, it may not perhaps be improper to assign the reason why the analysis of Laplace coincides exactly with the particular law of probability. It arises from the approximation employed by the French geometer; according to which the squares only of the errors are retained in the expressions of the probabilities, the higher powers being dismissed. There is thus the most perfect agreement between the results obtained both ways; a convincing proof, in point of fact, that the two methods are fundamentally the same, and are different only in the mode of investigation.

March 2, 1825. JAMES IVORY.

XXVI. *An Account of the Earthquakes which occurred in Sicily in March* 1823. *By Sig. Abate* FERRARA, *Professor of Natural Philosophy in the University of Catania, &c. &c.*

[Concluded from p. 100.]

Physical Observations.

WHEN the people about Ætna perceived their houses beginning to shake, they turned their eyes towards the volcano, and waited in expectation of an immediate eruption. And while they looked, fearful apprehensions filled their minds, and they prayed that the event, be it what it would, might take place at once.

The philosopher, who observes the phænomena of nature for the sake of reducing to the same class those of an analogous origin, and thence to deduce them from the same cause, observes the link which connects earthquakes with volcanic operations, and sees with the ignorant vulgar those mighty forces preparing in the subterranean furnace, which are able to put in motion immense masses of the solid globe, and to agitate them as water is agitated by a violent wind. The eruption of Ætna in 1811 was interesting from the grandeur of the spectacle which it presented, and no less so from the instruction which it conveyed to the naturalist. A new opening was made on the surface of the mountain. Explosions of tremendous force preceded the emission of immense columns of smoke and inflamed masses of matter, which were incessantly belched out towards heaven, and whose approach was announced by horrid roarings and explosions which filled the air to a great distance. Each explosion was accompanied by shocks; and as the interval between them was but of a few minutes duration, the city and country to a vast extent were in a continued undulation. For many days at Catania, eighteen

teen miles distant, we were rocked as though we had been upon the sea. Some of the shocks were very violent. The door of my chamber, which I left purposely a-jar, kept a continued beating against its side posts. The shocks lasted as long as the volcano was in operation; and when the external phænomena disappeared, the internal fire not being yet extinguished, deep subterranean rumblings and explosions were heard, and shocks felt at each report.

When the fire invests substances, it rarefies their masses to a great degree; the acquisition of new volume produces a proportionate expansion; and under the action of an enormous accumulation of inflamed matter, a passage is made for it with sudden and fearful energy. The expansion of water, for example, under a medium pressure of the atmosphere, is 1728 times its first volume, and it increases in the ratio of the heat. At 110° of Reaum. the pressure is equal to four atmospheres only. The explosion of a single barrel of gunpowder shocks and overthrows the whole vicinity. If, then, a subterranean stream of water happens upon places where volcanic fires are burning, it is at once converted into steam, acquires a density proportioned to the resistance of the mass of earth above it, circulates about, and agitates the most solid mountains and great tracts of land, until, losing its heat in the cavities of the earth, it returns to the state of water, without having given any external marks of its existence. It seems that the return of the terrible phænomenon is owing to the flow of water into places on fire—of water, the streams of which are determined only by accidental causes.

The vast furnace in the interior of the earth being inflamed, the fire attacks every thing exposed to its influence: some are liquefied, while others are converted to vapour; these, developing their volumes, form a system of force moving with immeasurable power. The subterranean cavities, little able to contain them, are violently convulsed in all their dimensions; and this effect is transmitted by the solid earth to distances proportioned to the quantity of force, to the transmissive power of the body moved, and to various local circumstances favourable or otherwise to the propagation of motion. After having combated with the obstacles which oppose roaring under the earth like the winds of Æolus, to find an outlet from the places in which they were produced, they circulate in various canals, until a cold temperature deprives them of the heat which gave them such power, and they sink into their former state. Often, however, they drive before them the matter which the heat has liquefied; and urging it towards the ancient mouths of

volcanos, belch it out in flaming rivers in the midst of the terrible phænomena which they themselves produce*.

Urged by the passion for observation, I have often descended into the horrid cavity of the crater, and approached near the blazing brink of the new orifices which have vomited forth streams of fire in my own time: I have seen immense torrents of aqueous vapour urged from the vast chimney, whose base is lost in the deep furnaces below; I have been bathed in the water to which the vapour was reduced by the low temperature of the atmosphere into which it entered; often have I seen it fall in fine showers all around me. Having penetrated into the recesses of the globe, it is in this manner forced out again by the heat to which it is exposed. I have observed the hydrogen gas, one time burning with its peculiar colour, at another, bursting forth with a loud deep explosion; the sulphuric and muriatic vapours whitening the immense clouds of smoke, and filling all the air with their suffocating breath; or, seizing upon the solid substances around, remaining fixed upon them. Fused substances, forced up by the elastic vapours, are disgorged from the same mouths, spread about in torrents of fire, and consolidated by the contact of the air. Is it not possible that the seat of these products may be extremely deep, and that yet they may reach the surface? Who knows but in other places, those grand laboratories of nature, from causes which will always elude our investigation, may be so deeply seated, that their productions never arrive at the surface; and that no other evidences of their existence, no other effects of their action are perceptible, than the shaking of the earth, and the rumblings which the aëriform elastic vapours make in the cavities of the earth†.

* In my "Description of Ætna," I have proved that the furnaces to this volcano cannot be under the foundation of the mountain, but at various distances from it. The immense vaults, which must have been formed after so many ages of conflagration, would, at the first violent shock, have swallowed up the whole mountain; and the combustible materials would have been exhausted in so small a circumference. The inflamed matter in different situations, from causes established by long usage, flows towards Ætna, and is ejected by it. Seneca (*Epis.* 79) acknowledged this truth; "ignem in ipso monte non alimentum, sed viam habere."

† The deficiency of volcanos in any place ought not to be made an argument against the existence of igneous fermentation under that place; since it may be placed at a great depth, or at least not be sufficiently large to form an eruption. And indeed, notwithstanding the numerous volcanos which have burnt at one time or other, in almost every region, may it not be possible that there is still but one great reservoir of fire, the remains of that which in remote ages has burst out in Portugal, Spain, the South of France, Italy, the islands of Great Britain, Germany, Bohemia, about the Bosphorus, on the Coasts of Asia, and in many other places?

Three principal furnaces have their outlets on the three sides of Sicily, and each with a force proportioned to the circumstances which supply it with combustible matter. Ætna, on the eastern side, by the immensity of its power rules the whole island. When in full action, the island trembles to its foundation, and feels the mighty power which has borne rule there from time immemorial. Its roarings are heard from one extremity to the other; but the parts most agitated are those in its neighbourhood and those between it and Cape Passora, a space of about a hundred miles.

The mountain of Sciacca, on the southern shore towards the west, seems to cover a place where the elements have been in ceaseless operation for ages. From dark caverns which open in the more elevated parts, torrents of water in the form of heated vapour, with sulphurous gases, are ejected. Having penetrated into the internal recesses, but unable to extinguish the fermentation, the water becomes invested with fire, is converted into vapour, and thus exhaled into our atmosphere. The extrication of the steam causes in the internal caverns a deep roaring, and often fearful convulsions felt at a great distance. At such times Sciacca, at the foot of the mountain, experiences the most violent commotions. In 1578 it was reduced to ruins. In 1652 for fifteen days it suffered the most severe and unremitted shocks. For some months in 1724 the earth was so frequently and violently agitated that all the inhabitants fled into the country. In September 1726 all the western part of Sicily was shaken with the greatest severity; and in Palermo at that time many lives were lost and many edifices destroyed. In June of 1740 Sciacca felt 22 shocks, with injury to buildings and loss of lives; that of the 25th was of such immense force that it extended as far as Palermo. After the middle of December 1816 the inhabitants heard extraordinary rumblings under the mountain; and in January of the succeeding year the shocks were so frequent, that 12 were sometimes counted in one day, and so violent that it seemed that the foundations of buildings must be rooted up—the rumblings and explosions under the mountain became fearfully loud—and the sea dashed in great waves against the shore at its foot. Sambuca, 15 miles distant, suffered much injury. A strong odour of sulphur pervaded the air all about Sciacca. While nature was in this agitation in the western part of the island, the eastern was enjoying perfect quiet. Over against Sciacca, at the distance of 70 miles, Pentellaria rises from the sea, and presents the same phænomena: an island of lava and other burnt matter, and streams of heated vapour of water and of sulphur issuing incessantly from its

 cavities.

cavities, show a great fermentation in the deep caverns under the sea, and to which little is wanting to renew its ancient conflagrations. Off the northern coast of Sicily is situated a chain of islands extending from east to west and terminating with Ustica, at the distance of 42 miles from the western shore of Palermo. All of these islands, sons of volcanic fire, which has raised them from under the depths of the sea, bear the impressions of the terrible element; and some are still burning, and serve as outlets to the subterranean furnaces. Vulcano, 22 miles from Cape Milazzo, burns, roars, thunders, and belches out continually immense columns of smoke and flame. Stromboli ceases not a moment in vomiting forth smoke, flame, and streams of vapour, which, rushing from the inflamed mouth, produce a horrible roaring, spreading terror among all the Eolian islands and the adjacent coasts of Sicily and Calabria. Lipari still preserves in its baths a part of that heat which one day fused into glass the matter of which it is formed. The action of these islands has almost always troubled Sicily. Early one morning, in February 1444, enormous masses of heated matter, amidst huge volumes of smoke and flame, were raised from the summit of Vulcano, and hurled about the sea to the distance of six miles, while strong shocks agitated this island and Sicily*. Other flaming masses were thrown out on the 24th of August 1631, which driven by the wind passed over Naso in Sicily, directly in front of Vulcano; and on the next day this unhappy city, by the violence of the convulsions of the earth, was entirely laid in ruins. Many persons were injured. A cleft was made in the soil, from which a very strong odour of sulphur issued†. On the 22d of April 1717, at dawn of day, a deep subterranean murmur was heard, accompanied by a severe earthquake, the shocks of which were felt all along the northern shore even to Messina. But the places which suffered most were those nearly over against Vulcano, as Milazzo, Pozzodigotto Castrorealo, 26 miles distant from it. The last city was entirely ruined‡. Shocks were renewed in the same places in 1732; and with much greater force in 1736, when the whole northern coast was violently affected, particularly Palermo, Ciminna, which was much damaged, and Naso, which suffered still more§. On the 4th of May 1739, about 5 o'clock P.M. the inhabitants of St. Marco, a town at the back of Naso, saw thrown from the mouth of Vulcano immense clouds of smoke and burning matter, which, driven by the wind, came roaring and thundering over Sicily, letting fall perpendicularly into the sea and on the neighbouring shore

* Faz. dec. 1.

† Car. Dial. il Bonan.

‡ Bott. de Trin. ten. Mess. 1717.

§ Mong. Stor. dei trem.

flaming

flaming matter, which gave out on every side bright sparks and struck with fearful crashes. It passed over Naso and St. Marco, and went on wasting itself in the interior. Such phænomena were unlucky omens to these unhappy towns. At 12 o'clock on the 9th a dreadful howling from Vulcano was followed by a violent shock, which after a few moments was repeated with many explosions; more than a hundred were counted within six days, and another on the 21st. Great rocks were detached from the mountains in the vicinity. Another flaming mass on the 9th of June darted from Vulcano and passed over Sicily; shocks were felt till the 22d, accompanied by howlings and numerous explosions from the burning mountain. St. Marco suffered exceedingly; but Naso was entirely destroyed*. The volcanos of Eolia contributed much to the earthquakes of Calabria and Messina in 1783. Stromboli was almost always in great commotion. For many days it seemed like a mad bull, which, raised above the waves, by his roaring filled Calabria and Sicily with terror. Vulcano often accompanied it, and its deep rumblings and vast columns of smoke and flame were terrible.

After the violent earthquake of Sciacca in 1816 the same evil fortune happened to other parts of the island. On the 15th of April 1817 a severe shock terrified the people of Caltagirone in Valdinoto, and of the neighbouring places. One happened at Catania in October, and another on the 20th of February of the following year, 1818, which was enormous. All the towns about Ætna were ruined, and many lives lost. Catania felt its injurious effects. It was felt all over the island, since at Palermo it produced three undulations. Others which followed it, and which continued to agitate Catania and the neighbouring region until April, were felt with greater force. All these shocks were the precursors of the grand eruption of Ætna, which burst out on the 27th of May 1819, and which lasted until August. While Sicily was trembling, this volcano was making its preparations in silence. The effects of the operations of Ætna are felt in places at a great distance from the mountain. After the troubles of February and April, Catania and its vicinity enjoyed repose until the 8th of September, when all Madonia was convulsed. Other shocks succeeded in October and November. On the 25th of February 1819 a very severe one was felt, which extended to a great distance. At Palermo three motions were produced, the last of which was very violent. The shocks in the whole of the vast extent of the mountains, where so much injury was done to the houses of the

* Amico Auct. ad Faz. Mong. l. c.

numerous

numerous inhabitants of these regions, were always preceded and followed by subterranean murmurs and distant explosions. Under these places it seems that those substances were deposited which Ætna inflamed and ejected from its mouth in the following May; because, after the eruption commenced, Madonia was left in quiet, while Ætna, which till this time, and during the agitations of Madonia, had remained perfectly calm, became convulsed with earthquakes. They accompanied the eruption.

With the extinction of the conflagration in August all the phænomena ceased, and the earth was no longer agitated. But in 1822 Ætna showed that the fermentation within its furnaces was again at work. On the 5th of April rumblings and continued explosions were heard, which were followed by great clouds of smoke violently driven from the crater by the impetuous current of elastic vapours. A shower of sulphurous ashes fell all around. On the 6th a violent shock convulsed all the towns between Ætna and Madonia—Capizzi, Cesarà, Sperlinga, Troina, Gangi, Gagliano; but in the midst of these Nicosia seemed the centre of impulse in all the shocks which followed throughout the month. Its soil appeared on the point of being torn up by force; many buildings were destroyed, and its inhabitants fled in consternation to find an asylum in the country. The immense clouds of smoke and earthy ashes which were ejected from June to October—which covered the more lofty part of the mountain with a gray stratum—which filled the atmosphere and gave out through the whole region a strong odour of sulphur, clearly prove that all these commotions were produced by forces collected in the recesses of Ætna*.

While

* From June to October 1822 Ætna emitted great quantities of volcanic ashes, which were scattered all over the mountain; on the plain about the crater it fell to the depth of a foot. From the mouth of the crater and through fissures near the mouth, so dense a smoke and such copious streams of aqueous vapour were given out, that when they were condensed by the lower temperature of the air the ground about these orifices was drenched with water. The vapour, which was still suspended by the caloric imparted to it by that already condensed, fell soon after in the form of a brine, acidified by the mixture of sulphurous vapour contained in the smoke, and to which was owing the odour of sulphur given out by the ashes wherever it fell. All the ashes about the crater was saturated with this brine. The vapour of water is always found in the smoke of Ætna, but in much greater quantities at the time of an eruption. In my relation of that of 1792, I mentioned that at a little distance from the crater a new orifice was made by the force of the vapour, from which for a long time pieces of old lava and scoriæ and argillaceous earth saturated with water, were ejected; that standing there to observe it, I was continually bathed in the brine which fell from the smoke. This phænomenon of Ætna in 1822 has been much misrepresented in foreign journals,

While Nicosia and the whole space between Madonia and Ætna were in such commotion, Sicily to the west and all the northern coast enjoyed perfect quiet; but a sad reverse was preparing. In October, Ætna ceased throwing out sulphurous ashes and sand, and with it ceased all its noises and shocks, and all was calm. In February at the beginning of the next year small motions of the earth were felt along the northern side of the island, which were the preludes to the scene that presented itself in March.

The direction of the motion was from N.E. to S.W., as was proved by all the phænomena mentioned in the beginning. I will not be guided by the injuries suffered in different parts,

journals, which say, that in the recent eruptions of Ætna the earth opened at a great distance from the crater, and that a muddy substance which is not lava was thrown out. As this important error, should it gain credit, would be injurious to science, I make all haste to correct it. In 1822, neither at a great nor at a small distance from the crater, the earth opened, and the matter thrown out is volcanic ashes, perfectly like that which has usually been expelled by this volcano; at least for the forty years that I have studied it. It did not come out in the form of mud, but in exceedingly fine dust, which afterwards became wet with the vapour condensed within the very edges of the fissures, or which fell in brine. It is a long while since any of the writers on volcanos, wishing to establish the theory of "eruptions of mud," have named that of sea-water and shells in 1755; a popular credulity, which I have been compelled to do away by every possible proof. This new error of 1822 might recall their arguments and lead on to other errors. I have given with much pleasure a true detail of the fact to the illustrious M. de Humboldt, who wrote me on the subject with that ardent zeal which characterizes him, and which has rendered him, as he is proclaimed in both hemispheres, one of the greatest observers of nature. With respect to the nature of these volcanic ashes, although I am convinced that it differs not at all from that which has always been ejected, yet I wished to consult the oracle of chemistry upon it, since it is his delight to discover the composition of bodies; I mean the illustrious Vauquelin, whose kind regard to me has conferred on me so much honour. My first packet, much to my regret and that of the eminent chemist who was expecting it, never reached its destiny; but I renewed it, and the results shall have place in my continuation of the History of Ætna from 1818, where I left it, which I shall soon publish. I will add, to finish this note, that the "muddy eruptions" so called by our Macalubbi, are not such, even according to the imaginary ideas of Plato, who admitted rivers of mud in the interior of the globe, to which end he alleged such eruptions in Sicily. Nothing comes up from the depth of the earth but streams of carburetted hydrogen gas, which finding above the argillaceous chalk of which the soil is formed, loosened away by rain-water, it forces it up and causes it to flow in muddy streams. In times of drought, dust only is forced up, and in its passage a whistling is made like an impetuous wind. Even of our lake of the Palici they believe that the water comes from the interior of the earth, and wonder that it never overflows. Why do they not observe that in dry years it entirely evaporates, and that nothing comes out of the chinks at its bottom but currents of air, which give to the water the appearance of boiling when it collects there from the rain.

for

for these spring from a complication of causes; from the soil, its greater or less capacity of receiving and communicating motion, from the manner in which it presents itself to the progressive motion, and from the state of the edifices. These circumstances may sometimes produce anomalies which easily deceive those who do not bestow in the examination of them the attention which they deserve: but without fear of error I may say, that in general the shock was much the most forcible on the northern shore, and at a little distance from it; and that it went on gradually diminishing towards the interior. The moving force, then, must have been in operation somewhere under the sea opposite this part of the island. Naso was almost entirely ruined; Patti, and all the towns about Capes Orlando and Calava, and which are nearer Eolia, were considerably damaged. Some very small thinly inhabited towns lost little, because they had little to lose; others were in some measure defended by their situations. Palermo, at the bottom of a bay which curves towards these burning islands, and surrounded by large and high mountains on the other side, was exposed to the whole force of the motion against it: this it was, together with the degraded state of its buildings, which brought such ruin upon this beautiful city. Every thing seemed then to announce to us that the most expansive vapours which proceed from the burning furnaces of Eolia, in developing their immense volumes, urged against the sides of those cavities which once contained the matter of which all these islands are formed, produced the motion that struck obliquely against Sicily, and moving along the shore towards the west, spread despair throughout Palermo. After the shock of the 5th, their motion was more free; and they were heard murmuring under the soil near our island, seeking an outlet from the obscure caverns in which they were generated, but not propagating their motion to any considerable distance. The course of that of the 7th was in the same direction with that of the 5th; but that of the 31st was in a direction directly opposite; since it was felt at Messina, and not at Palermo. The undulations were determined by the horizontal direction of the motion; the perpendicular shocks, by a force acting from below upwards, which supposes a much greater depth in the situation of the acting force than the other, without ever being in any case nearer the surface. Every one may easily distinguish the difference which subsists between the superficial motion caused by the rapid passing of a heavy carriage, or by the sudden combustion of a large quantity of confined gunpowder, which would cause the darting of a large accumulation of electric fluid to restore the equilibrium between

tween the earth and the atmosphere, were it possible for it to collect in the midst of so many conducting bodies, which seem designed to restore the equilibrium instantly;—between this motion, and the deep heavy earthquake, armed with such terrible power, which agitates so violently a great extent of the globe, which sometimes seems ready to tear it from its very foundation, and which has all the characters of an effect sprung from most wonderful degrees of force, which, placed deep in the earth, moves and convulses those great masses lying between it and the surface.

The idea of forces and effects like these fills with fear the miserable mortal who creeps upon the face of the earth, and brings his pride down to the dust. When he sees the earth reel, and the great fabrics which he has raised with so much confidence rushing to ruin, he despairs of finding any where one firm support to his frail existence.

The chinks and fissures formed in many places, and to which the vulgar attribute much importance, are in consequence of the quaking of the soil, and to which the softness of the earth and the loss of its internal support have given place. The country of Bosco about Ogliastro, of which I have already spoken, became furrowed with divers long, tortuous, deep clefts, the sides of which in some places sank down; in other places, portions of the surface passed down over inclined plains below them, and took new positions; the olive-trees, which some of these carried with them, were much injured by the breaking and displacing of their roots. This land is formed of an immense deposit of argillaceous chalk, more than a hundred feet deep. The water which penetrated it (and the winter there was very rainy) loosened away the earth, and carried a great part of it into the internal cavities below; the surface thus wanting solid support, under the shock of the earthquake became filled with depressions, caverns, and inequalities. The same may be said of a great aperture made in the vicinity of Colesano, which dilating itself day after day threatened to render those places inaccessible. Copious showers alone produce such effects in the chalky land of many parts of Sicily. This want of firm bases frequently causes the overthrow of great rocks at the time of earthquakes. Well do we remember, that in the earthquake of the 5th of February 1783, a mountain a mile to the south of Scilla, and which was a mile and a half in length, fell over into the sea of Calabria and formed two new promontories.

Phænomena observed in the Eolian Sea.

If all these facts induce us to locate in Eolia the causes of the

the physical events of the past March, it is necessary to inquire if these islands exhibited at that time any phænomena which may corroborate our opinion. I will mention, therefore, in this place many facts, about which there can be no uncertainty, and which will be of the greatest importance should any one wish to push the suspicion which I have announced in this memoir to certain evidence*.

Since September of last year the daily quantity of smoke from Vulcano has been much greater than usual, and flame has often been visible in the evening. Explosions have been frequently heard on the neighbouring coasts of Sicily. But Stromboli has exhibited the greatest activity for almost fourteen months without intermission. Shocks have been very frequent, and so strong as to fill the islanders, although accustomed to them, with great apprehensions. The island, with the blazing mountain itself, seemed often on the point of being torn up from its foundation. The volcano opened two new mouths on the side which looks towards the sea, and belched out from them fearful clouds of sand and burning rocks, which after darkening the air fell to the earth. Fortunately their direction was not towards any of the little habitations or cultivated fields of the island. One forest only on the side of the mountain suffered some injury. The inhabitants often found themselves enveloped in thick clouds of black smoke and ashes, which the wind drove among them. But only one man was struck by the burning rocks hurled through the air with immense violence. The scoria and ashes did much damage to the cisterns of the island, and to the terraces which serve as tiles over them. Torrents of black smoke, ashes and sand were often ejected and thrown to various distances. The greatest shocks were sometimes followed by a thick dry cloud which filled the air of the whole island.

The shock of the 5th of March was very strong at Stromboli, at Saline (formerly Didime), and at Lipari. The inhabitants of Lipari did not doubt that their houses would this time be reduced to ruins; and they have not yet ceased giving thanks to Heaven and their protecting saints for defending them from utter destruction. They affirm that a moment after the shock all their thoughts were turned upon the disasters which might happen to places on the neighbouring coast of

* The external phænomena of a volcano show that the effects of the fermentation have come to the surface; but Nature operates often in the dark recesses of the earth, without exhibiting any external visible effects of her operations: elastic vapours may form there, shake the soil, and return to their concrete state. When eruptions happen from the inflamed mouths, it is because these subterranean forces have met with substances which may be thrown out, thus giving certain proof of the existence of these forces.

Sicily

Sicily and at Palermo, towards which the direction of the motion seemed to be. Lipari lies between us and Stromboli. Since April the parts of our island which were before agitated have been left in repose; but shocks are still frequent at Stromboli, and keep the poor inhabitants there in continual fear. The subterranean furnace seems to have lost much of its power, as the elastic vapours generated there shake but a very limited space, and the new apertures of the mountains emit now and then but a very small quantity of fine sand, which is always the last product of an expiring conflagration.

From what I have laid down it is just to conclude, that the fires of Eolia are those which have for a long time been preparing the event of last March; that it was produced by motions generated in those mighty furnaces, and that those motions were propagated to great distances. If Sicily then is so often shocked, the powers which agitate it must exist in volcanos that burn within its own bosom and in the surrounding sea. Situated in the midst of such grand operations of Nature, Sicily must be exposed to all the effects which such powerful causes are capable of producing. The chemical subterranean operations require that the earth should every where be traversed by vast cavities and canals, running in various directions; and the forces of the operations act on the different parts of these cavities. But it is natural to believe, and many facts in this memoir demonstrate the truth of it, that places in the vicinity of the three great volcanic outlets ordinarily feel the force with the greatest violence. In this respect the situation of Palermo is very advantageous; since it is distant from Ætna and from Eolia, and is near to Sciacca only, which is the least energetic. And this grand and respectable city would be less exposed to such grievous disasters than all the other cities of Sicily, did its edifices possess that character, which they might easily be made to possess, which constitutes true solidity and firmness.

XXVII. *On Mr.* Sturgeon's *Suggestion relative to the Electric and Magnetic Causes of the Earth's Motion, and on its Similarity to the Theory of Mr.* W. Herapath. *By Mr.* J. P. Bevan.

To the Editor of the Philosophical Magazine and Journal.

Sir,

On opening your Magazine for October last, I was struck with the similarity between some of the observations of Mr. W. Sturgeon in his paper on electro-magnetism, published in that Magazine, and those of my friend Mr. W. Herapath,

rapath, delivered, in a lecture on that branch of science, before the Society of Inquirers*, on the 19th of April 1824. A report of this lecture was published in Felix Farley's Journal (a leading newspaper of this city) at the same time. In this report Mr. Herapath observes, "that as a body possessed of the electric and acted on by the magnetic fluids revolved upon its axis, it was rendered extremely probable that the earth's motion was occasioned by these fluids, the electric generated within itself by the chemical changes constantly taking place *from the action of solar heat;* we have only to conceive that there is a great preponderance of positive fluid at one pole, and of negative at the other, to satisfy the conditions of its ability to move."

In his lecture he stated that he had been induced to form this opinion in consequence of the following reasons: "A body filled with the fluids given off by chemical action and by the magnetic fluids, will revolve upon its axis, as exemplified by Faraday's revolving magnet; it will also, when thus filled, revolve round a fixed body similarly circumstanced, and its distance from it will be regulated by the quantity of the fluids it possesses: the fluids given off have the power of penetrating space, as demonstrated by the galvanic stream passing *in vacuo*. The motions of bodies revolving from the impulses enumerated are either from west to east, or from east to west, according to the nature of the fluids: thus, suppose a body acted on by negative electricity and north magnetic fluid in one direction, and by positive electricity and south magnetic fluid in the opposite, it ought to revolve upon its axis. The magnetic fluids are polarized in the earth; and reasoning from analogy, the electric should be so also; since we find that in all bodies containing electricity the positive appears at one end and the negative at the other: all the conditions, therefore, requisite for a body to move in an orbit round another, as well as to revolve upon its own axis, appear to be possessed by the earth. As the electric and magnetic fluids are the only *philosophical agents* which would produce rotary motion, and as the earth possesses these fluids, it is not unphilosophical, in the absence of demonstration, to conclude that they are the cause of its motion."

These quotations prove that Mr. Herapath entertained the opinion that the electric and magnetic fluids were the cause of the motion of the earth long before Mr. Sturgeon, or at least that by priority of publication he is entitled to the honour

* I would here observe, that in this Society the members lecture to each other on the different departments of science, &c., at their own cost respectively.

nour which may attach to it; and as this is often all that the philosopher obtains, it should in justice be awarded to him to whom, as being the first publisher, it is unquestionably due; and that Mr. Herapath was so in the present instance you will be convinced from the report of his lecture, which I have taken the liberty to inclose.

I am, sir, yours, &c.

Bristol, Feb. 24, 1825. JAMES P. BEVAN.

XXVIII. *Demonstrations of Trigonometrical Formulæ.*

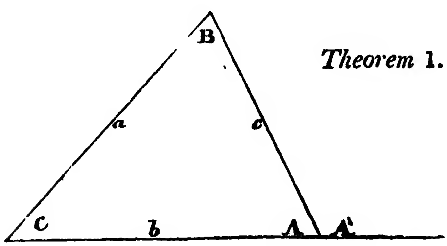
To the Editor of the Philosophical Magazine and Journal.

Sir,

I FLATTER myself the demonstrations of the following trigonometrical formulæ are *new:* but if I am mistaken, perhaps the circumstance of their not being in general use may procure them insertion in your Magazine.

I am, sir, yours, &c.

Lincoln's Inn, Feb. 14, 1825. C.



Theorem 1. Let ABC be a triangle having the angles A, B, C to which the sides opposite are a, b, c — Then by trigonometry,

$$a \sin C = c \sin A \text{ or } a \sin (A + B) = c \sin A$$

Hence $$\sin (A + B) = \frac{c}{a} \sin A$$

Again, if a perpendicular be drawn from C to the opposite side c, we have

$$c = a \cos B + b \cos A$$

$$\therefore \frac{c}{a} = \cos B + \frac{b}{a} \cos A = \cos B + \frac{\sin B}{\sin A} \cos A$$

Hence by substitution

$$\sin (A + B) = \left(\cos B + \frac{\sin B}{\sin A} \cos A\right) \sin A$$

and $\therefore \sin (A + B) = \sin A \cos B + \cos A \sin B$

Theorem

Theorem 2. If a perpendicular be let fall from B to the opposite side b, we have

$$b = a\cos C + c\cos A$$
$$= c\cos A - a\cos(A+B)$$

Hence $\cos(A+B) = \frac{c}{a}\cos A - \frac{b}{a} = \frac{c}{a}\cos A - \frac{\sin B}{\sin A}$

Now $c = a\cos B + b\cos A$

$$\therefore \frac{c}{a} = \cos B + \frac{b}{a}\cos A = \cos B + \frac{\sin B}{\sin A}\cos A$$

Hence by substitution

$$\cos(A+B) = \left(\cos B + \frac{\sin B}{\sin A}\cos A\right)\cos A - \frac{\sin B}{\sin A}$$
$$= \therefore \cos B\cos A + \frac{\sin B}{\sin A}\cos^2 A - \frac{\sin B}{\sin A}$$
$$= \cos B\cos A + \frac{\sin B}{\sin A} - \sin B\sin A - \frac{\sin B}{\sin A}$$
$$= \therefore \cos B \,.\, \cos A - \sin B\sin A$$

Theorem 3. Let A' be an exterior angle of the triangle. Then by Euclid $c = A' - B$.

Also $a\sin C = c\sin A = c\sin A'$

or $a\sin(A'-B) = c\sin A' \therefore \sin(A'-B) = \frac{c}{a}\sin A'$

Now $c = a\cos B + b\cos A$
$$= a\cos B - b\cos A'$$

$$\therefore \frac{c}{a} = \cos B - \frac{b}{a}\cos A' = \cos B - \frac{\sin B}{\sin A}\cos A'$$
$$= \therefore \cos B - \frac{\sin B}{\sin A'}\cos A'$$

Hence by substitution

$$\sin(A'-B) = \left(\cos B - \frac{\sin B}{\sin A'}\cos A'\right)\sin A'$$
$$= \therefore \sin A'\cos B - \cos A'\sin B$$

Theorem 4. Since $b = a\cos C + c\cos A$

or $= a\cos(A'-B) - c\cos A'$

$$\therefore \cos(A'-B) = \frac{b}{a} + \frac{c}{a}\cos A'$$
$$= \frac{\sin B}{\sin A} + \frac{c}{a}\cos A' = \frac{\sin B}{\sin A'} + \frac{c}{a}\cos A'$$

Now $c = a\cos B + b\cos A = a\cos B - b\cos A'$

$$\therefore \frac{c}{a} = \cos B - \frac{b}{a}\cos A' = \cos B - \frac{\sin B}{\sin A}\cos A'$$
$$= \cos B - \frac{\sin B}{\sin A'}\cos A'$$

Hence

Hence by substitution

$$\cos(A' - B) = \frac{\sin B}{\sin A'} + \left(\cos B - \frac{\sin B}{\sin A'} \cos A'\right) \cos A'$$

$$= \therefore \frac{\sin B}{\sin A'} + \cos B \cos A' - \frac{\sin B}{\sin A'} \cos^2 A'$$

$$= \quad \frac{\sin B}{\sin A'} + \cos B \cos A' - \frac{\sin B}{\sin A'} + \sin B \,.\, \sin A'$$

$$= \therefore \cos B \cos A' + \sin B \sin A'$$

XXIX. *A new binary Arrangement of the* Macrurous Crustacea. *By* A. H. Haworth, *Esq., Fellow of the Linnæan and Horticultural Societies of London, and of the Imperial Natural History Society of Moscow, &c. &c.*

[Continued from p. 106.]

To the Editor of the Philosophical Magazine and Journal.

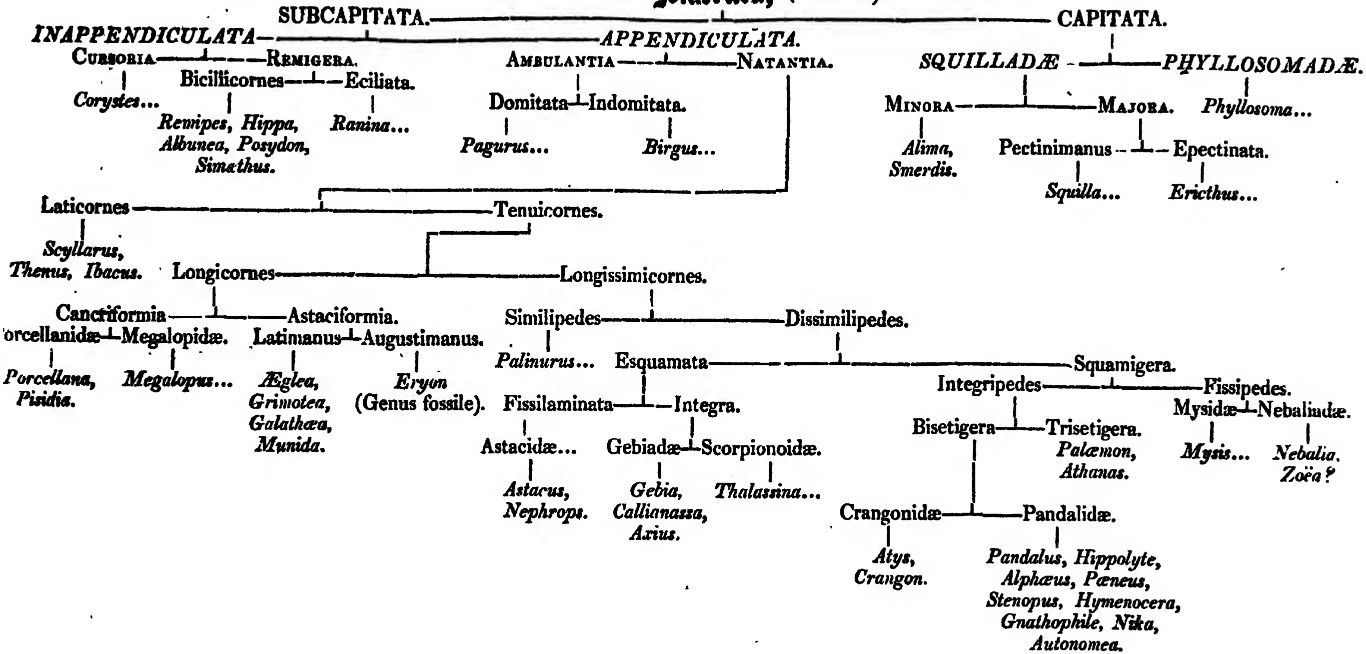
Sir,

HEREUNDER I transmit you the promised continuation of my new binary arrangement of crustaceous animals, including the great branch Machura; which, if you could print in the manner of my first and second tables, would much better display the horizontal analogies and affinities than my last or third table can; the *first* articles of every dichotomy in any given group having ever the *least* affinity to each other, and the last articles the *most.* Wherefore the said first articles (as animal—vegetable) are often merely *analogies* (in point of relationship to each other); but these insensibly, as we go down the table and arrive at the *genera,* lessen and blend into the closest *affinities.* But I allow that the mode in which you have printed my last or third table shows more advantageously to a mere reader, the current *location,* or natural distribution of the *genera;* which, notwithstanding what is in modern, not old times, supposed to the contrary, I now believe to be *a continuous one,* that is unravelable in the way of a straight line; but arising dichotomously from one root (which I have termed Matter) and proceeding in a repetitely double, and, in point of magnitude or quantity, unequal series; resembling as it were an inverted branching and exuberant tree, whose foliation and flowers may be compared to our artificial genera, and natural, and therefore permanent species.

As soon as I can procure some books which must be consulted before I can proceed much further with this business, you shall have the requisite continuations; and in the mean time I remain, sir, yours, &c.

Queen's Elm, Chelsea, March 1825. A. H. Haworth.

Macrura, *(continued)*.



XXX. *On the Geology and Topography of the Island of Sumatra, and some of the adjacent Islands. By the late* William Jack, *M.D. M.G.S.**

THE various journeys which have recently been made into the interior of Sumatra have considerably extended our hitherto very imperfect knowledge of the geography of that island, and have also furnished materials for a slight outline of its geological structure. Some account of the information which has been acquired on both these topics may not prove unacceptable as an accompaniment to a small collection of specimens of the rocks of that country, which I have transmitted to the Geological Society.

I obtained these chiefly from the western coast of the island, which, from its greater boldness and proximity to the mountains, is the most accessible to mineralogical investigation. It is to the eastern side that all the great rivers take their course, where the extent of alluvial land is consequently much the most considerable. If any reliance can be placed on native traditions, the increase of surface on this side, by alluvial deposit, has been great and rapid; as in all their earliest histories the town of Palembang, which is now at least sixty miles from the mouth of the river, is mentioned as a sea-port, and the adjacent hill of Siguntangguntong as an island. The smoothness of the inland seas, into which these rivers flow, must certainly favour the accumulation of the earthy particles which they wash down.

On the western side, a difference of character and aspect may be remarked between that portion of the island which lies to the north of Indrapore and the southern. The former comprises about two-thirds of the length of the island, and includes the richest and most interesting districts; its coast is more irregular and broken, and is defended by innumerable small islands; while the hills, at one time approaching towards the shore, at another receding from it, appear to pursue no determinate line. In the southern portion the coast is but slightly indented, and is skirted by few islands; while the hills run in nearly a continuous chain, as far at least as Bukit Pugong near Croee, at a distance of from ten to twenty miles from the coast, and form what is usually called the Bukit Barisin or Barrier range. In the northern portion there is much less appearance of parallelism in the distribution of the hills; and though the whole of Sumatra may, in a general view, be considered as forming a great chain parallel to that of the

* From the Geological Transactions, New Series, vol. i. Part II. p. 397.

Malayan peninsula, its parts by no means exhibit a corresponding regularity. In this particular the greater number of maps convey an erroneous idea.

It has been ascertained that the Poggy islands, Pulo Nias, and the whole of the northern coast, are laid down in Horsburg's charts considerably to the westward of their true position, a circumstance which materially affects the supposed breadth and form of the island; and it is singular that, with the exception of Acheen Head, Bencoolen, and perhaps Flat Point, there is scarcely a place on the west coast (where we have had establishments for above a century) of which either the latitude or longitude is exactly determined. The mountains, for the most part, lie nearer to the western coast than might be supposed from Marsden's map; and a greater length of course must, therefore, be given to the great eastern rivers, which have their sources among them. It has been an object of interest in our late investigations to trace more correctly the course, the relative position, and true sources of these noble streams, most of which afford safe navigation to the largest vessels for upwards of a hundred miles above their mouths.

The basis of the island of Sumatra is probably primitive; granite has been found in Menangkabau and at Ayer Bangy: but trap rocks are perhaps the most widely diffused; while the mountains of greatest elevation, and which stand in some degree insulated, are generally volcanic. The volcanos of Sumatra have somewhat a different character from those of Java: the former generally terminating at the summit in a ridge or crest; while the latter are more exactly conical, and have for the most part much broader bases.

Commencing at the north with Acheen [respecting which the mission of Sir T. S. Raffles in 1819 afforded the means of adding to our information] I have merely to remark, that the mountains which terminate in Acheen Head, together with the adjacent island of Pulo Way, and the coast to the eastward, including Pedier, are of calcareous formation.

Proceeding southwards along the western coast we come to the Bay of Tappanooly, which forms a large and deep indentation among the hills of the Batta country. In these hills, which come directly down to the margin of the sea, as well as in the small islands within the circumference of the bay, the rocks consist chiefly of a fine-grained sandstone, frequently striped with various shades of yellow and red. The strata are in general even and regular, and but slightly inclined, occasionally, however, exhibiting partial disturbances and undulations.

At Nattal, the next station to the southward, the mountains recede

recede to some distance from the coast, leaving a portion of level land, through which the river pursues a winding course to the sea. There is a small detached hill near its mouth entirely composed of limestone, of which a great quantity in loose blocks and fragments is strewed at the bottom. The soft argillaceous iron ore, commonly called ruddle, has also been procured from the upper part of the river.

Inland of Tappanooly and Nattal lies the country of the Battas, in which the position of the great lake of Tobah, not laid down in our maps, has lately been determined. It lies about fifty miles north-east of Tappanooly.

The Batang Tava and Sinkuang have been represented as inconsiderable streams, while they are in fact among the largest rivers on the western coast; the former having its source in the mountains of Diri, to the north of Tappanooly, and the latter rising in Gunong Kalaber, the southern boundary of the Batta country, and watering in its course the whole province of Mendheling. The Tabuyong, on the other hand, which is represented by Mr. Marsden as the largest river between Tappanooly and Nattal, is in fact small, and does not penetrate beyond the first range of hills.

The province of Mendheling, which lies inland of Nattal, has long been celebrated for its gold, which is of the finest quality. It is said to possess upwards of seven hundred mines, and its annual export of gold probably does not fall short of a thousand tales.

At Ayer Bangy, where the hills again approach the sea, granite makes its appearance. This place lies nearly due east from Gunong Pasaman (known in our charts by the name of Mount Ophir), a remarkable conical mountain, whose height was some years ago estimated at 13,800 feet above the level of the sea, its latitude at 6′ north, and its distance from the coast about 26 miles. On its north-eastern side is the source of the great river of the Soompoor or Rukan, which crosses the island in a north-easterly direction, passing through the fertile valley of Rau, at its exit from whence it bursts through the range of mountains forming the eastern boundary of that country; and after precipitating itself over a considerable fall enters the district of Rukan, from which it receives its name in this part of its course.

To the southward of Rau lie the provinces of Agam, Rana-lima-pulo, and Menangkabau, which, collectively distinguished by the appellation of the "Darat," or "the Land," constituted the ancient empire of Menangkabau. In the territory which was formerly included in that empire the population is at present estimated at not less than a million and a half; and

some of the villages or towns are several miles in circumference. The Siak river, which is navigable for a hundred miles from its mouth, rises in the northern portion of Rana-limapulo, and chiefly from the mountain Tinkalang.

With Menangkabau and the sources of the Indragiri river the journey undertaken in 1818 by Sir T. S. Raffles has made us better acquainted. He proceeded from Padang, on the west coast, and after crossing three ridges of hills, exceeding 4000 feet in height, and covered with primeval forests, through which the beds of torrents afforded the only passage, he descended upon the Tigablas country, which is bounded on the south by Gunong Tallang. The cultivated part of the great valley of Tigablas may be about twenty miles long and ten broad, and would seem to have been at one time entirely covered by the waters of the existing lake, which is skirted by hills in every direction, that of the valley excepted. Gunong Tallang, with its adjacent hills, seems to form a transverse range, and to break the regularity of all the other ranges which it intersects. On the eastern side of the lake, which is about fifteen miles long by from seven to nine broad, commences the province of Menangkabau Proper, and at the distance of a few miles from its banks is situated the ancient capital of Pagaruyong.

The Indragiri river has its source on the eastern side of Lake Sophia*, and flows through the province of Menangkabau, receiving the waters of the celebrated Ayer Mas or "Golden Stream," which passes through Pagaruyong, and soon after of a large river which rises in Agam, behind G. Singalang and G. Berapi, and traverses a portion of the more eastern district of Rana-lima-pulo. The Indragiri is navigable for small boats a considerable way above the falls; but the exact position of these has not been ascertained. The mountain of most interest in this quarter is that of Berapi, which is constantly emitting smoke: its elevation was ascertained, by angles taken from the lake, to be about 13,000 feet: it is connected towards the western coast with the mountain Singalang, estimated at about 12,000 feet; and to the north and eastward with Gunong Kasumbra, first discovered on this expedition, and calculated to be not less than 15,000 feet above the level of the sea, being therefore the highest mountain in Sumatra. The Kampar, which is mentioned in the Portuguese Histories as of some importance, is a river of small size, situated between the

* The lake, to which the natives had given no proper name, has lately received the denomination of Lake Sophia, in honour of Lady Raffles, who accompanied Sir T. S. Raffles in his late expedition into this part of Sumatra.

Siak and Indragiri; it has its origin in the easternmost hills which bound the province of Rana-lima-pulo, and does not penetrate beyond them into the more elevated country of the "Darat."

As a detailed account of the journey to Menangkabau, and of the observations made during the course of it, will probably be given by Dr. Horsfield, who accompanied Sir T. S. Raffles on that occasion, I shall here only remark generally, that granite was observed on both sides of the lake; sometimes passing into gneiss and micaceous schistus, and at others associated with marble and limestone, or with sandstone; that basaltic and trap rocks were abundant; and that obsidian, lava, and pumice were observed in the valley of the Tigablas.

The country to the southward of Padang, as far nearly as Indrapore, is a confused assemblage of hills, which come directly down to the sea; and the whole coast is broken into innumerable bays and islands. Through the greater part of these the mountain masses of rock are composed of a kind of trap or amygdaloid, containing, in a cement of a grayish-brown colour, numerous small fragments or nodules of other rocks, so firmly united that the fracture takes place through both indiscriminately, and so hard as often to ring under the hammer. Padang head is chiefly composed of trap; it also furnishes pebbles of chalcedony and large crystals of quartz.

From Indrapore to Bencoolen the range of hills runs nearly parallel to the coast, leaving a belt of lower, but not level land between them and the sea, whose action has exposed a long range of cliffs composed of a stiff dark-red clay. Behind the first range of hills to the east of Moco Moco lies the country of Korinchi, in which there is a considerable lake, which was first visited by Dr. Campbell in 1800, and by him named Lake George. It was again visited by the directions of Sir T. S. Raffles in 1818, and the party proceeded as far as Penkalan Iambi. From the observations then made, it appears that the lake is much nearer the western coast, and more to the southward than is laid down by Marsden; and that a cultivated valley lies to the north of it, watered by a small river, which descends to the lake from Gunong Api, a high volcanic mountain constantly smoking, and distant about sixty miles north-east of Indrapore point. The small lake noticed by Marsden, which should also have been placed to the northward, was dried up about ten years since, by the effect of an earthquake. Lake George gives out a considerable stream at its southern end, which, passing through the district of Penkalan Iambi, becomes one of the principal branches of the river Iambi.

The

The countries of Limun and Batang Assii, through which the two southernmost branches of this river pass, abound in gold, which has latterly been chiefly exported to Moco Moco, Bencoolen, and Palembang.

At Bencoolen the line of hills is situated about twenty miles inland; and the space between them and the sea exhibits a succession of ridges and ravines, whose general direction is parallel to the coast, though frequently altered and broken by the irregular working of springs and streams. The hills are principally of the trap formation, and exhibit several varieties of basalt, whinstone, and much of the same gray amygdaloidal trap observed in the neighbourhood of Padang. The most remarkable hill in this quarter is detached from the Barrier range, and is called Gunong Bungko, or by Europeans the Sugar-loaf. I had lately an opportunity of ascending it, and found it composed almost entirely of irregular masses of basalt or trap, whose bare surfaces are frequently exposed, rising from amid the luxuriant vegetation of the declivities. It had not previously been explored, the shape of the hill rendering the ascent extremely difficult. Its elevation is less than 4000 feet; yet towards the top the trees became stunted, the rocks were clothed with dense moss, and the vegetation assumed a character decidedly Alpine. In the beds of some of the rivers near Bencoolen, particularly that of Silebar, are found pebbles of jasper and chalcedony, with nodules of indurated clay. Iron ores occur not unfrequently, and in one part of the Bencoolen river a bed of coal is laid bare by the stream. Very fine specimens of siliceous petrified wood have also been found in the hills of the interior.

It was not till 1818 that a journey was accomplished across the island of Sumatra in any part. In that year a party proceeded from Bencoolen to Palembang; and the facilities of communication are now found to be such that, did not political obstacles intervene, it would no doubt soon become a frequent channel of intercourse between Bencoolen and the more eastern parts of the Archipelago.

The Pasummah country, which was first visited by Sir T. S. Raffles in 1818, is an extensive plain of remarkable fertility, considerably elevated above the sea, as may be inferred from the temperature, the thermometer being usually as low as 65° at 10 A.M. From this plain rises Gunong Dempo, which towers above all the mountains of this part of Sumatra, and is estimated to be no less than 12,000 feet above the level of the sea. It is almost constantly emitting smoke; and hot springs and other volcanic phænomena are common in its neighbourhood. It has been ascended since Sir T. S. Raffles's visit;

vegetation

vegetation was found almost to cease near the summit, and a large portion of it bore evident marks of a late and violent eruption. The cold was extreme, and the ascent difficult; but, owing to a mistake of the guides, the party did not reach the present crater. The hills which separate Pasummah from Mannæ, and indeed the whole barrier range from Bencoolen to Cawoor, are composed of basalt or trap: from the plain of Pasummah I have specimens of quartz abounding with masses of iron pyrites.

In the interior of the Lampong country is a lake, the position of which was not sufficiently determined to enable Mr. Marsden to lay it down in his map. This lake has been twice visited within the two last years by the orders of Sir T. S. Raffles: from its neighbourhood are obtained jasper, slate rock, and trap; there is a hot spring on one side of it. It gives origin, near its southern extremity, to a river which, after passing through the country of Haji, takes the name of Kamring, and falls into the Palembang river a little below the town.

Tulang Bawang rises in Gunong Ompo, to the southward of lake Ranau, and has no communication, as was formerly supposed, with the Palembang.

Among the islands which skirt the western coast of Sumatra the largest and most important is that of Pulo Nias, which has hitherto remained almost unknown to Europeans. It is about seventy miles long by twenty-five broad, and is for the most part hilly, though none of its mountains are of great elevation. It is very populous, and its soil, which is naturally rich, is highly cultivated. The most singular circumstance in its geological structure is the extensive occurrence of calcareous masses of coral origin, which are found near the surface on almost all the hills, lying immediately above the rocky strata, and to all appearance precisely in their original position *. These coral masses are in general so little altered that their different species can be determined with certainty, and even the fragile stems of the *Madrepora muricata*, and other branched kinds, may be found no otherwise injured than by the pressure of the superincumbent soil, and the constant infiltration

* The specimens of coral which were received from Pulo Nias, and which were collected from the hills by Dr. Jack, as appears from the labels attached to them, are all rounded masses, evidently water-worn; and they may be divided into two classes. The 1st appears indeed exactly to resemble recent coral; the structure of the coral is unaltered, and the cells of the polypes empty, and consequently the rock is of the usual lightness of coral. The 2d class consists of a calcareous rock of the ordinary specific gravity of limestone: in specimens Nos. 19,105, and 19,106 the rock resembles some

filtration of water through it. The species are obviously the same with those which now abound under the neighbouring sea, and sometimes the transition from the recent to the fossil coral is only effected by the gradual rise of the land from the shore. Large Kima shells (*Chama Gigas*) are also found on the hills, exactly as they occur on the present reefs, and are collected by the inhabitants for the purpose of cutting into rings for the arms and wrists. Every thing seems to indicate that the surface of the island must at one time have been the bed of the ocean, and that, by whatever means it attained its present elevation, the transition must have been effected with little violence or disturbance to the marine productions at the surface. The subjacent rocks are stratified; among them I found granular quartz, limestone, and calcareous sandstone*: towards the southern end I also met with several kinds of limestone, some a coarse yellowish-white stone, and others of a blueish cast, with small fragments of shells intermixed.

At one place at Tallo Dalam I found strata of a calcareous rock, laid completely bare, on the crest of a hill, dipping to the north-east, with an inclination of above 45°, and abruptly broken on the other side into a kind of stair. I have a specimen of this rock†, which contains fragments of shells and fossil wood.

The appearance of unchanged and unfossilized masses of coral on the surface of the hills seems most readily explicable

some of the oolitic beds of Europe; and there are dispersed through it faint traces of coralline bodies, which probably indicate that it has been originally derived from corals; but in general the substance of the coral has been almost entirely dissolved, and the pores of what remains filled up by sparry matter. This specimen has the aspect of an ancient rock. Another specimen, No. 19,104, exhibits very faint traces of coralline bodies; but the rock is filled with innumerable irregular cavities, which are stained internally by oxide of iron: the specific gravity of this last is also that usual to limestone. The rounded shape of the specimens, Nos. 19,104, 19,105, 19,106, leads us to infer that they were not procured from a rock *in situ*, but were perhaps found lying on the surface, or imbedded in the soil of the hills: if so, they may be considered as detritus; and may therefore very possibly be portions of solid strata existing in the island. The specimen of recent coral, stated to be found in the hills, is water-worn, and appears to have been a detached piece. – *Note by the Secretaries.*

* It is highly deserving observation, that these rocks, particularly No. 19,099, have a striking resemblance to parts of the green sand formation in England, especially to the rock called Kentish rag; and it is also worthy of remark, that in Sumatra, on the part opposite to Pulo Nias, at Nattal-hill, a rock occurs exactly corresponding to No. 19,099: this bed, therefore, probably extends across the channel that separates Pulo Nias from Sumatra.—*Note by the Secretaries.*

† This specimen, No. 19,103, is probably from one of the lower beds of the island; it bears a considerable resemblance in its aspect to one of the rocks of the oolitic series.

on

on the supposition, either of a subsidence of the ocean below its original level, or a heaving up of the island by a force from beneath. If we admit the former of these causes, indications of a similar subsidence ought to be found on the adjacent coasts: but I am not aware of any such having been observed. The great inclination of the strata, and the dislocation they sometimes appear to have suffered, would seem to favour the latter hypothesis. It must still however be regarded as a phænomenon of a most singular kind, that so large an island, diversified with numerous hills from 800 to 3000 feet in height, should have been heaved up from the sea with so little disturbance to the fragile marine productions on the surface.

The appearance and nature of these productions would indicate a comparatively recent date to the event.

The other large islands of the chain, Pulo Batu, Mantawi, and the Poggies, are less known, but are probably not very dissimilar in structure to Pulo Nias: they are not nearly so populous or so well cultivated; but the quantity of sago and cocoa-nuts which they produce sufficiently proves that they are not deficient in fertility and natural resources.

The islands on the eastern side of Sumatra are of two descriptions: those which lie off the mouths of the Siak and Indragiri rivers, on the western side of the Straits of Malacca, are merely alluvial flats; while the islands of Banca, Lingen, &c., may more properly be considered as belonging to the Malayan chain, and as a continuation of the range which forms the peninsula of Malacca, being similar to it in geological situation, and in their mineral products, the most abundant and remarkable of which is tin*.

XXXI. *On the Structure of the Tarsus in the Tetramerous and Trimerous Coleoptera of the French Entomologists. By the Rev.* W. Kirby, *M.A. F.R.S. & L.S. &c.*

To the Editor of the Philosophical Magazine and Journal.

Dear sir,

FROM the abstract you have given in your last Number, of a paper of my friend Mr. W. S. MacLeay's, On the structure of the tarsus in the tetramerous and trimerous *Coleoptera* of the French entomologists, read at the Linnæan Society, it appears that he was not aware of its having been long ago discovered

* In the collection of rocks sent by Dr. Jack from Sumatra occurs a specimen, No. 19,345, of soft white chalk, *Creta scriptoria*, containing the fragment of an echinus. No mention of it is made in the memoir, nor was any information contained in the label attached to it, except the locality, Bencoolen.—*Note by the Secretaries.*

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by De Geer that the tarsus of *Coccinella*, usually regarded as *trimerous*, is really *tetramerous;* for, speaking of the third or claw-joint, he says: "Cette partie qui a une *articulation* près de son origine," &c.; and this accessary joint he has also figured*. Mr. Spence, in a letter to me dated July 13, 1809, observes that Müller had discovered this joint in that genus; and further remarks, "But on examining the *Lepturæ*, *Cerambyces*, and *Chrysomela tenebricosa*, I find (what had escaped Müller) that the claw-joint of these also has a similar minute joint at the base; so that if this be allowed to be a true joint, these have in truth *five-jointed* tarsi." Mr. Spence, however, did not regard this as a true joint, or one having separate motion. Upon receiving this intimation I made a pretty general examination of tetramerous beetles, and found that the *Chrysomelæ* of Linné in general have this joint, as likewise *Curculio* L., and others; I discovered also that it is a true joint, moved by muscles of its own. In consequence the following passage will be found in the third volume of the Introduction to Entomology, some time printed.

"*Pentamerous* insects are those which have *five* joints in all their tarsi. This is the most universal, and may be called the *natural* number of these joints†."

"*Tetramerous* insects are those in which all the tarsi consist of *four* joints; these in the *Coleoptera* are next in number to the pentamerous—indeed a very large proportion of them, strictly speaking, are really of the latter description, since in Linné's four great genera, *Curculio*, *Cerambyx*, *Chrysomela*, and *Cassida*, and some others, the *claw-joint* (*Ungula*) consists of *two* articulations, one very short, forming merely the ball at its base‡, which inosculates in the socket of the preceding joint, and the other constituting the remainder: if you carefully separate these two pieces, you will find that the last inosculates in the summit of the ball, and is moved by appropriate muscles§. This structure probably permits the readier elevation and depression of this joint ||."

Mr. MacLeay is too candid, I am sure, to feel any objection to your following the old adage, *suum cuique*, on this occasion, and giving a place in your valuable Magazine to these remarks.

I am, dear sir, yours, &c.

Barham, March 15, 1825. — Wm. Kirby.

* De Geer. v. 364. t. xi. f. 6. *h*.
† Introduction to Entomology, iii. 683.
‡ Introd. to Ent. iii. Plate xxvi. fig. 47, 48. *d**.
§ Ibid. fig. 49. *s* · a.
|| Ibid. 684.

XXXII. *On*

XXXII. *On the Use of Analysis in investigating the Elementary Relations of Figure and Forces.* By A CORRESPONDENT.

To the Editor of the Philosophical Magazine and Journal.

Sir,

IN a former short paper I pointed out a method by which some objections to Legendre's mode of evolving the properties of figure from functional equations might be obviated; and I resume the subject in the hope of clearing away certain misrepresentations, by means of which a sect of our philosophers have sought to bring the *principles* of such methods of inquiry into discredit. There is amongst one class of scientific men in this country a violent prejudice against the use of analysis, either in the elements of geometry or of mechanics. They regard it, not as the mere substitution of one mode of reasoning for another, but as a dangerous attempt to remove from the view the experiments upon which these sciences are built, and to revive the justly exploded philosophy of Leibnitz and his immediate successors. I will not stop to speculate respecting the secret biases on which this prejudice rests, although such speculation might be productive of some amusement, but hasten to examine the character of its ostensible foundation. It may be found now and then appearing in the Edinburgh Review*, in the shape of invective against the generalizations, &c. of the continental mathematicians: but it is preferable to apply at once to the fountain head, and to seek an official indication of it in the works of the great leader of this sect, Professor Leslie. From them the spirit will be obtained in its wildest and most unrectified state, and the exemplifications of it are as numerous as it is wild.

The following three sentences of the concluding paragraph of his note respecting Legendre will amply serve my purpose. "In this abstruse research assumptions are still disguised and mixed up with the progress of induction. Such indeed must be the case with every kind of reasoning on mathematical or physical objects which proceeds *à priori*, without appealing at least in the first instance to external observation. Of this kind are some of those ingenious analytical investigations respecting the laws of motion and the composition of forces." With the second sentence of this extract I conceive that every philosophic mind will now-a-days concur. There cannot be evolved from any train of reasoning one single truth which was not previously infused into it. Reasoning

* See, amongst other places, the notice of a memoir of Laplace's in the second volume of the *Mémoires de la Société d'Arcueil.* I might also have referred to some papers in the Supplement to the *Encycl. Britannica.*

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merely changes the forms of the conceptions on which it operates, or combines a number into one general expression. But the question is, with respect to the applicability of such remarks to the subject on which Mr. Leslie had just been commenting. Does Legendre proceed *à priori*, or attempt to deduce the primary properties of extension *without appealing in the first instance to external observation?* I certainly think not; and it will not be difficult to show cause for my opinion. —Whence, I would ask, is his fundamental equation derived, if not from superposition or *experiment?* and what more is necessary than this single appeal, to give the object he contemplates its complete definition? The thirty-second Prop. of Euclid is immediately traceable to the fourth and to the twelfth axiom. Legendre involves the axiom, and derives his equation from an equivalent to the fourth; and there is thus no *constitutional* difference betwixt the processes of the geometer and the analyst. But it will be further evident if we view the subject in its most elevated light.

A triangle analytically considered, is merely one of a class of functional equations which represent a relation betwixt six quantities, three of which are heterogeneous to the other three; or it is a particular case of the equation

$$a = \varphi\,(b, c, A, B, C, \gamma)$$

Now there will always be a limit to the number of possible ways in which such a set of quantities can be connected together. This limit is fixed in geometry, for each particular case, by what Euclid terms *definitions*. It is fixed by the *visible figure*, whose existence the definition declares *possible;* and the continued use of this figure prevents the reasoner from falling into the supposition of impossible combinations. The same limit is fixed in analysis, with the utmost generality, by the *rational form* which the foregoing equation necessarily assumes.

$$\frac{a}{b} = \varphi\left(\frac{b}{c}, \frac{A}{B}, \frac{B}{C}, f\gamma\right)$$

The use of visible figure, then, and the operation of the law of homogeneity are identical in this respect, and we shall quickly recognize the entire sameness of the other parts of the processes. The analyst and the geometer are at this point in precisely similar states. The geometer, in *assenting* to his definition, has assented to the possibility of the existence of relations betwixt certain quantities; and the analyst, in writing the foregoing equation, has done the same thing. But the great inquiry into the *nature* of these relations, in each case, is now before them. The geometer has to translate into the language of intellect the visible figure under his contemplation;

tion; or, to speak in plainer terms, he has to ascertain some fact or property containing the complete definition of the relations he has recognised: and the analyst has to search for circumstances which may lead him to the determination of ϕ. In the former case the definition must be obtained in the only manner in which the character of the visible world can be made known to intellect, viz. by *experiment:* and since ϕ is merely the symbolic representation of a figure, nothing definitive of it can be *primarily* obtained, excepting from that figure, and consequently from experiment also. From the circumstance that the equation determining ϕ must be one with six values, the analyst knows that at least three of the quantities included in ϕ must exist *co-indeterminately*—superposition informs him that *only* three can exist in such a state: and this, in union with his experimental knowledge of the abstract quantities a, b, c, A, B, C, is sufficient to guide him in his varied transformations and final evaluation of ϕ. Such then are the two methods, agreeing most scrupulously in the minutest circumstance affecting the *principles* on which they rest, and differing* only in the different artifices by which they particularize, transform, and combine the experimental facts. If the prejudice to which I have alluded had not operated more forcibly than any prejudice should operate within the cold and calm regions of science, this nice philosophical agreement had never been overlooked, and the invidious and unfounded distinction never drawn.

In reverting to the extract from Leslie's Geometry, I give immediate assent to the remark respecting the famous analyses of the laws of motion by Euler, D'Alembert, &c. They are essentially unphilosophical and fallacious. But I am not disposed either to extend the same agreement to the remark relative to the composition of forces, or to recognise the *philosophical* accuracy of the investigation which the learned Professor has himself given of it. It is remarked somewhere by D'Alembert, that the more abstract an investigation is made, the more perspicuous and satisfactory does it become. However paradoxical this may appear, it is nevertheless true. The nearer that any investigation approaches to an abstracted in-

* The only distinction that can be drawn *à priori* betwixt the two methods respects the comparative facility of the analytical. The geometer as he proceeds has to invent his logic or his methods of transformation; whereas the analyst has merely to apply the same treatment which he has been accustomed to give to similar cases. Perhaps too the course of particularizing a more general functional equation than I adopted above would lead to a scientific arrangement of the objects of geometry. The present arrangement resembles the *artificial* classifications of natural history.

tellectual

tellectual process, the less must be the chance of error. The use of experiment is merely to *completely define* the objects of contemplation. When the relation termed a triangle, for instance, is once defined by experiment, the evolution of its different forms is accomplished by reasoning alone, and it would be next to ludicrous were an editor of the *Elements* to offer experimental proofs of them. The experimental principles of every department of science should be reduced to the *fewest possible;* and the processes that furnished them ought not to be again introduced (excepting for mere *verification*), until the occurrence of some physical action *not already involved* by them. Does the composition of forces then involve any physical phænomenon not included amongst those susceptibilities of matter already defined by *the laws of motion?* The simultaneous action is quite analysable, and its analysis will instantly answer the question. One of the forces may be supposed to have already acted upon the body, and set it in motion. At a point in its course it is affected by the other force. Now this second force may be conceived decomposed into two others, one of which acts equally and oppositely to the force which has impressed the motion. At the instant of this action, then, the first force will be annihilated, and the body will be constrained by a new force acting upon it in another direction. The physical phænomenon of the joint action thus involves nothing new. The body merely loses one tendency to motion and receives another; and the determination of the resultant should be obtained by contemplating the physical truths *already known.* That it does follow from a combination of them will instantly appear. Let two equal forces x act upon a point M, making an angle $2\,\theta$. The inertia of matter determines that the resultant z shall bisect the angle. Here

$$z = \phi\,(x, \theta, \gamma)$$

which is converted by the law of homogeneity into

$$^{*}\frac{z}{x} = \phi\,\frac{\theta}{\gamma}$$

or conceiving θ the ratio

$$z = x\,.\,\phi\,\theta$$

but z, and consequently $\varphi\,\theta$ become 0 when, and *only* when

* This transformation is accomplished in the same manner by Poisson. It is a blemish in Francœur's little treatise, that he has chosen to deduce it otherwise, and more circuitously. Laplace's investigation is very different from either Poisson's or Francœur's. The English student who cannot refer to the first section of the *Méchanique* may find it in Dr. Thomas Young's Illustrations, &c.

$\theta =$

$$\theta = \pm \frac{\pi}{2}$$
$$= \pm \frac{3\pi}{2}$$
$$= \pm \frac{5\pi}{2}$$
$$\cdot \; \cdot \; \cdot \; \cdot$$
$$= \pm \frac{(2n+1)\pi}{2}$$

These indicate the factors of the cosine: hence

$$\phi \theta = a \cos \theta$$

and $\qquad z = a x \cos \theta$

Again, when $\qquad \theta = 0 \quad z = 2x$

Hence finally $\qquad z = 2x \,.\, \cos \theta$

It will be seen that this is the immediate result of the combination of the elementary ideas contained in the laws of motion. I am aware that it is the practice to preface the demonstration of this theorem with a set of *physical axioms:* but if the *inertia* of matter has previously been stated in its proper *extent*, the necessity of this is avoided. The investigation I have now given differs somewhat from those in celebrity. It differs from them probably in possessing superior simplicity, but in *principle* it is essentially the same. Poisson in discovering the equation $\varphi x \,.\, \varphi z = \varphi(x - z) + \phi\,(x + z)$ and in the evaluation of a, and Laplace in rejecting the forces $\frac{xy}{z}$ and $\frac{xy}{z}$, and in evaluing k and ϱ, involve all the physical ideas which I have now combined. The theorem of the composition of forces bears to these experimental facts precisely the same relation that the principle of gravitation bears to Kepler's empirical laws—they are both merely *condensed expressions.*

Although I consider that Professor Leslie has in both these questions been misled by an unaccountable dislike with which he seems to regard the approaches of analysis, and that in the latter question this dislike has drawn from him an inadequate exhibition of an important elementary truth, I cannot go along with *Dis-Iota* in the full extent of his censure. I cannot allow that Mr. Leslie has not been *eminently* successful in illustrating many elementary subjects; and were proof wanting, I would simply refer to his neat, or rather elegant exposition of the physical characters of fluids in the volume which has drawn forth a portion of these remarks. In this paper I have used those liberties with his name to the use of which an anonymous writer is entitled; but I have not laid aside

aside the feeling of respect. His name is coupled in my mind with that of Laplace when I think of capillary attraction. He has many other honours, and I am happy to know that he will soon have more. Professor Leslie is too far above an anonymous writer to be affected either by his praise or blame, but for that very reason I deem his errors dangerous. Now-a-days every great mind is bent on extending the boundaries of science. The questions and disputes of philosophy that surround its entrance are disregarded; and error, when recommended by *authority*, is apt to be received by the multitude without due examination. In such a state of things the contributions of a humble individual may not be unacceptable.

March 19, 1825. Σ.

XXXIII. *Method of extracting Titanium from Minerals, and of separating it completely from the Substances with which it is found combined.* By M. PESCHIER.

WHEN I made known in 1821 and 1822 the results of my analytical researches on the micas (*Journal de Physique*), and communicated in the following year to M. Vauquelin those which I had obtained from the talcs, I could not suppose that the process which I had followed was not exact, and that the proportions of titanium which I had stated were exaggerated; for the properties analogous to those of silica, alumina, magnesia, and lime, which this principle possesses, were not known to me: these earths also have sometimes been looked upon as pure when it was found united with them, and at other times that which was believed to be pure titanium was a mixture of titanium and of one or the other of these earths.

But having since discovered these different properties and the means of separating these principles with exactness, I occupied myself in rectifying several of my analyses. I communicated some of them this spring to our Society of Natural Philosophy and Natural History, and should not have made them known prior to the publication of the work on titanium with which I am occupied, if the note of M. Vauquelin on the presence of titanium in micas, inserted in the September Number of the *Annales de Chimie et de Physique*, p. 67, had not obliged me to anticipate this period; for, in discovering titanium in all the micas, this philosopher informs us *that those which contain the greatest quantity of it did not yield him a hundredth part.*

I shall first describe the order of the processes to be followed

* From *Annales de Chimie*, vol. xxvii. p. 281.

lowed in the analysis of a mineral with a base of titanium, and then relate some of the important results with which it has furnished me.

1. I treat the porphyrised mineral with two parts of potash: withdrawing the vessel from the fire when the mass is at a white heat, I mix the product in water, throw it on a filtre, and wash the insoluble residuum till the liquid no longer acts on test paper. To separate from the washings the small portion of titanium which is dissolved with the silica, I slightly supersaturate them, evaporate them to a humid saline consistence in a porcelain vessel, moisten the saline product in water, and receive it on a filtre, where the silica becomes deposited; then, after having washed and dried it, I expose it to the action of diluted oxalic or hydrochloric acid, to separate from it the titanium or any other substance which might be deposited with it. I next add the liquid from which I have separated the silica to the acid solution with which it had been acted upon; I treat them with the infusion of galls, render them slightly alkaline, concentrate them; and if they take the red-brown colour which characterizes titanium, I put them by, to examine at the end of the operations.

2. I submit the insoluble residuum in the potash to the action of hydrochloric acid, diluted with six or eight parts of water, with the aid of boiling: if a greater quantity of insoluble substance remains than would be expected, I treat it a second time with potash, and pursue with it the operations above stated. I then saturate the acid solutions with an alkaline subcarbonate; and after having separated the precipitate from it, I evaporate the liquid to a moist saline consistence, proceed with the deposit which may be formed there by the solution of the product in water, as with that of § 1; and finding the action of the infusion of galls on the washings as has been mentioned, I add them to the former solutions of the same nature, if it indicates the presence of titanium.

3. I expose the precipitate formed in the acid solution to the action of potash; and as the titanium dissolves wholly or in part with the alumine, and the hydrochlorate of ammonia, in leaving a portion of dissolved titanium, precipitates a great part of it with the alumine,—to avoid this cause of error, which was formerly unknown, I use in its place sulphate of ammonia, in which I have discovered the property of precipitating the alumine only; and when I have received and washed this earth on a filtre, I evaporate the liquids to a humid saline consistence; I separate, by solution of the product in water, some little of the silica which was dissolved in it; I throw some

 infusion

infusion of galls on the washings, and mix them with the two before mentioned.

4. The titanium not dissolving so easily in the potash as the alumine every time they meet, the insoluble residuum preserves a gelatinous character; and in order to separate it from the substances with which it is mixed, this residuum is dissolved in hydrochloric acid, by which treatment some portions of silica are also separated: the iron of this solution is precipitated by the hydrocyanate of potash and of iron; the liquid is then saturated with an alkaline subcarbonate, and it is carried to ebullition. The precipitate which becomes formed is white, bulky, and has the aspect of alumina: as it may consist of a mixture of titanium, magnesia, and lime, the first is rendered insoluble in acids by being strongly heated, and the earths are dissolved by digestion for some hours in a weak acid—say distilled vinegar. The insoluble parts are separated by a filtre; the liquid is treated with ammonia, in order to remove the magnesia, with oxalate of ammonia for the lime: and we know that the operation has been well conducted, if it does not afterwards undergo any change with the infusion of galls. To the liquids which have been put aside is then also added that from which the iron and other substances have been separated.

5. Lastly, as titanium forms double salts with all acids, and as the tannate of titanium is easily re-dissolved by the infusion of galls, we next obtain that which by these two causes always escapes analysis, by evaporating to dryness all the liquids which have been put aside, igniting the residuum, dissolving in water the resulting saline mass, throwing the solution on a filtre, washing the insoluble parts, heating them to redness in order to destroy the carbonaceous matter, and washing afresh in acidulated water the white powder they give, which is the titanium sought for. If it is found to be coloured by iron or manganese, it is easily rendered pure by digestion in nitromuriatic acid, after having exposed it to a very strong heat. By repeating this series of operations twice upon the washings, and adding each time some infusion of galls, all the titanium contained in the mineral under examination is extracted. I may state by the way, that considering its property of forming double salts, I have always separated several grains from the salts obtained in the examination of the alkaline principle in minerals of this kind; that its presence is known by the spongy state that the hydrochlorates of potash or of soda take, which, thoroughly deprived of ammonia and strongly ignited, do not enter into fusion; and that several solutions,

lutions, evaporations, and exposures to a very strong heat, are necessary to effect this purpose completely.

Such is the series of minute operations which I found it indispensable to follow in the analysis of minerals with a base of titanium (of which the number is much greater than is commonly supposed), and by the aid of which the black foliated mica of Siberia—which, according to Klaproth, is composed of silica 42·50, alumina 11·50, magnesia 9, oxide of iron 22, manganese 2, potash 10, loss by ignition 1; total 98—has furnished me silica 24, alumina 8·50, magnesia 5, peroxide of iron 30, manganese 0·70, titanium 21, potash 5·70, loss by ignition 2·75; total 97·65. The talcs, the chlorites, and the steatites gave me, by following the same process, from 0·19 to 0·30 of a substance which, like that I have designated by the name of titanium in mica, forms also, like the titanium extracted from rutile, a gelatinous mass, transparent and yellowish, by the evaporation at a moderate heat of its solution in the hydrochloric acid; furnishes like that, by the saturation of its solution in an acid, a white, gelatinous and very bulky precipitate; by the infusion of galls a yellowish precipitate, which augments by a slight supersaturation of the acid; becomes brown, and is in great measure dissolved by the addition of the re-agent, giving to the liquid the colour of blood; is soluble in the pure alkalies; forms double salts with all the acids; becomes insoluble in acids by the effect of a strong heat, and consequently possesses all the characters of titanium, with the difference only that with the infusion of galls it does not furnish an abundant orange-red precipitate, and does not always take a lemon-coloured tint by heat: but these anomalies, which appear to me of little importance, may be classed with many others which this substance presents.

Such are the reasons which have induced me to communicate this process. But as I am fully sensible of the force of an opinion given by a philosopher of M. Vauquelin's merit, I submit it to the experience of chemists more used to this species of research than I am, and shall gratefully receive any information on the subject that may be transmitted to me.

XXXIV. *Additional Experiments and Observations on the Application of Electrical Combinations to the Preservation of the Copper Sheathing of Ships, and to other Purposes. By Sir* Humphry Davy, *Bart. P. R. S.**

I HAVE already had the honour of communicating to the Royal Society the results of my first researches on the

* From the Phil. Trans. for 1824, Part II. Read June 17, 1824.

 modes

modes of preventing the chemical action of fluid menstrua, such as saline solutions, or sea-water containing air, on copper, by the contact of more oxidable metals*.

For some months I have been engaged in a series of new experiments on this subject, so important to the navigation and commerce of the country: and through the liberal and enlightened views of Lord Melville and the Lords of the Admiralty, who desired the Commissioners of the Navy Board and of the Dock-yards to give me every assistance in their power, and all the facilities which our magnificent naval establishments at Chatham and Portsmouth furnish, I have been enabled to conduct my operations upon a very large scale.

At this advanced period of the session, it will be impossible for me to give more than a very short notice of experiments which have been tried under a great variety of circumstances, and the details of which would occupy some hours in reading; but I cannot deprive myself of the pleasure of stating the satisfactory and conclusive nature of the results, many of which have even surpassed my expectations.

Sheets of copper, defended by from $\frac{1}{40}$th to $\frac{1}{1000}$th part of their surface by zinc, malleable and cast iron, have been exposed, for many weeks, in the flow of the tide in Portsmouth harbour, and their weights ascertained before and after the experiment. Where the metallic protector was from $\frac{1}{40}$ to $\frac{1}{110}$, there was no corrosion nor decay of the copper. With smaller quantities, such as from $\frac{1}{200}$ to $\frac{1}{400}$, the copper underwent a loss of weight, which was greater in proportion as the protector was smaller: and as a proof of the universality of the principle, it was found that even $\frac{1}{1000}$th part of cast iron saved a certain proportion of the copper.

The sheeting of boats and ships protected by the contact of zinc, cast and malleable iron in different proportions, compared with those of similar boats and sides of ships unprotected, exhibited bright surfaces, whilst the unprotected copper underwent rapid corrosion, becoming first red, then green, and losing a part of its substance in scales.

Fortunately, in the course of these experiments, it has been proved that cast iron, the substance which is cheapest and most easily procured, is likewise most fitted for the protection of the copper. It lasts longer than malleable iron or zinc; and the plumbaginous substance which is left by the action of sea-water upon it retains the original form of the iron, and does not impede the electrical action of the remaining metal.

* Phil. Trans. 1824, Part I. or Phil. Mag. vol. lxiv. p. 30.

I had

I had anticipated the deposition of alkaline substance in certain cases upon the negatively electrical copper. This has actually happened. Some sheets of copper that have been exposed nearly four months to the action of sea-water, defended by from $\frac{1}{35}$th to $\frac{1}{80}$th of their surface of zinc and iron, have become coated with a white matter, which on analysis has proved to be principally carbonated lime and carbonate and hydrate of magnesia. The same thing has occurred with two harbour boats, one of which was defended by a band of zinc, the other by a band of iron, equal to about $\frac{1}{35}$th of the surface of the copper.

These sheets and boats remained perfectly clean for many weeks, as long as the metallic surface of the copper was exposed; but lately, since it has become coated with carbonate of lime and magnesia, weeds have adhered to these coatings, and insects collected on them: but on the sheets of copper defended by quantities of cast iron and zinc, bearing a proportion below $\frac{1}{150}$th, the electrical power of the copper being less negative, more neutralized, and nearly *in equilibrio* with that of the menstruum, no such effects of deposition of alkaline matter or adherence of weeds have taken place; and the surface, though it has undergone a slight degree of solution, has remained perfectly clean; a circumstance of great importance, as it points out the *limits of protection*, and makes the application of a very *small quantity* of the oxidable metal more advantageous in fact than that of a larger one.

The wear of cast iron is not so rapid but that a mass of two or three inches in thickness will last for some years; at least the consumption in experiments which have been going on for nearly four months does not indicate a higher ratio. This must, however, depend on the relation of its mass to that of the copper, and upon other circumstances not yet ascertained (such as temperature, the relative saltness of the sea, and perhaps the rapidity of the motion of the ship); circumstances in relation to which I am about to make decisive experiments.

Many singular facts have occurred in the course of these researches, I shall mention some of them that I have confirmed by repeated experiments, and which have connexions with general science. Weak solutions of salt act strongly upon copper; strong ones, as brine, do not affect it; and the reason seems to be that they contain little or no atmospheric air, the oxygen of which seems necessary to give the electro-positive principle of change to menstrua of this class.

I had anticipated the result of this experiment, and upon the same principle of some others.

Alkaline

Alkaline solutions, for instance, impede or prevent the action of sea-water on copper, having in themselves the positive electrical energy which renders the copper negative. Lime-water even, in this way renders null the power of action of copper on sea-water*.

The tendency of electrical and chemical action being always to produce an equilibrium in the electrical powers, the agency of all combinations formed of metals and fluids is to occasion decomposition, in such an order that alkaline, metallic, and inflammable matters are determined to the negative part of the combination; and chlorine, iodine, oxygen, and acid matters to the positive part. I have shown in the Bakerian lecture for 1806, that this law holds good in the Voltaic battery. The same law applies to these feebler combinations. If copper in contact with cast iron be placed in a vessel half full of sea-water, and having its surface partially above that of the water, it will become coated with carbonate of lime, carbonate of magnesia, and carbonate of soda; and the carbonate of soda will gradually accumulate till the whole surface in the air is covered with its crystals:—and if the iron is in one vessel, and the copper forming an arc with it in another, and a third vessel of sea-water in electrical connexion by asbestos or cotton is intermediate, the water in this intermediate vessel continually becomes less saline; and undoubtedly, by a continuance of the process, might be rendered fresh.

I shall not take up the time of the society by referring to some obvious practical applications of these researches to the preservation of finely divided astronomical instruments of brass by iron, of instruments of steel by iron or zinc: my friend Mr. Pepys has already ingeniously taken advantage of this last circumstance, in inclosing finely cutting instruments in handles or cases lined with zinc, and many other such applications will occur. I cannot conclude without mentioning particularly my obligations to Sir Byam Martin, the Comptroller, and Sir Robert Seppings, the Surveyor of the Navy, for the interest they have taken and the zeal they have shown in promoting these researches; and without stating how much I owe to the care, attention, and accuracy of Mr. Nolloth, master shipwright, and Mr. Goodrich, mechanist in the Dock-yard at Portsmouth, in superintending the execution of many of the experiments.

* I am at present engaged in applying this principle to experiments on the preservation of animal and vegetable substances.

XXXV.

XXXV. *Observations on the Application of Machinery to the Computation of Mathematical Tables. By* Charles Babbage, *Esq. F.R.S., &c. &c.**

SINCE I had the honour of communicating to the Astronomical Society a short account of an arithmetical engine for the calculation of tables, which has been examined by several of the members of this society, I have not added much to the practical part of the subject. I have however paid some attention to the improvements of which the machinery is susceptible, and which will, if another engine is made, be greatly improved.

The theoretical inquiries to which it has conducted me are however of a singular nature; and I shall take this opportunity of briefly explaining to the society some of the principles on which they depend, as far as the nature of the subject will permit me to do this without the introduction of too many algebraic operations, which are rarely intelligible when read to a large assembly.

Of the variety of tables which are required in the present state of science, by far the larger portion are intimately connected with that department of it which it is the peculiar object of this society to promote.

The importance of astronomical science, whether viewed as the proudest triumph of intellectual power, or considered as the most valuable present of abstract science to the comfort and happiness of mankind, equally claims for it the first assistance from any new method for condensing the processes of reasoning or abridging the labour of calculation. Astronomical tables were therefore the first objects on which I turned my attention, when attempting to improve the power of the engine, as they had formed the first motive for constructing it.

I have already stated to the society, in my former communication, that the first engine I had constructed was solely destined to compute tables having constant differences. From this circumstance it will be apparent that after a certain number of terms of a table are computed, unless, as rarely happens, it has a constant order of differences, we must stop the engine and place in it other numbers, in order to produce the next portion of the table. This operation must be repeated more or less frequently according to the nature of the table. The more numerous the order of differences, the less frequent will this operation become requisite. The chance of error in such computations arises from incorrect numbers being placed in the engine: it therefore becomes desirable to limit this

* From the Memoirs of the Astronomical Society, Part II. vol. i. p. 311.

chance

chance as much as possible. In examining the analytical theory of the various differences of the sine of an arc, I noticed the property which it possesses of having any of its even orders of differences equal to the sine of the same arc increased by some multiple of its increment multiplied by a constant quantity. With the aid of this principle an engine might be formed which would require but little attendance, and I believe that it might in some cases compute a table of the form A sin θ from the 1st value of $\theta = 0$ up to $\theta = 90°$ with only one set of figures being placed in it.

It is scarcely necessary to observe what an immense number of astronomical tables are comprised under this form, nor the great accuracy which must result from having reduced to so few a number the preliminary computations which are requisite.

In pursuing into its detail the principle to which I have alluded, which lends itself so happily to numerical application, I have traced its application to other species of tables, and am enabled to point out a course of analytical investigation which will in all probability afford ready methods for constructing tables, even of the most complicated transcendent, in a manner equally easy.

I will now advert to another circumstance, which, although not immediately connected with astronomical tables, resulted from an examination of the engine by which they can be formed.

On considering the arrangement of its parts, I observed that a different mode of connecting them would produce tables of a new species altogether different from any with which I was acquainted. I therefore computed with my pen a small table such as would have been formed by the engine had it existed in this new shape, and I was much surprised at discovering that no analytical method was yet known for determining its *n*th term. The following is the first series I wrote down:

	Series.	Diff.		Series.	Diff.		Series.	Diff.
0 ...	2	0	10 ...	222	42	20 ...	924	86
1 ...	2	2		264	46		1010	86
	4	6		310	46		1096	92
	10	6		356	52	25 ...	1188	100
	16	12	15 ...	408	60		1288	108
5 ...	28	20		468	68		1396	114
	48	28		536	74		1510	114
	76	34		610	74		1624	118
	110	34		684	78	30 ...	1742	120
	144	38	20 ...	762	80		1862	122
10 ..	182	40		842	82		1984	

The equation of finite differences from which it is produced is

$$\Delta^2 u_x = \text{units fig. of } u_{x+1}$$

which

which is one of a class of equations never hitherto integrated. I succeeded in transforming this equation into a more analytical form: but still it presented great difficulties; I therefore undertook the investigation in a different manner, and succeeded in discovering a formula which represented its *n*th term. It is the following:

$$u_z = (\bar{a}) + 20b\,(10b + 2a - 1)$$

TABLE.

0	2
1	2
2	4
3	10
4	16
5	28
6	48
7	76
8	110
9	144

where $(\bar{a})$ represents the number opposite a in the annexed subsidiary table, and a is the figure in the unit's place of z, and b is that number which arises from cutting off the last figure from z. Example: let the 17th term be required, then $z = 17$, and $a = 7$, $b = 1$; the number opposite 7 in the table is

$$(\bar{7}) = \quad . \quad . \quad . \quad . \quad 76$$

$$10b + 2a - 1 = 10 + 14 - 1 = 23$$

$$20b = 20 \qquad 20b\,(10b + 2a - 1) = \underline{460}$$

$$536 = u_{17}$$

I have formed other series of the same class, and have succeeded in expressing any term independent of all the rest by two distinct processes. Thus I have incidentally been able to integrate the equations I have mentioned: I will just state one other of a simple form; it is the equation

$$\Delta\, u_z \text{ units fig. } u_z$$

whose integral is $\qquad u_z = 20b + 2^a$

where a is that one of the numbers 1, 2, 3, 4, which taken from z leaves the remainder divisible by 4, and b is the quotient of that division.

The table is as follows:

1 —	2
	4
	8
4 —	16
	22
	24
	28
8 —	36
	42

 One

One of the general questions to which these researches give rise is, supposing the law of any series to be known, to find what figure will occur in the *k*th place of the *n*th term. That the mere consideration of a mechanical engine should have suggested these inquiries is of itself sufficiently remarkable; but it is still more singular, that amongst researches of so very abstract a nature I should have met with and overcome a difficulty which had presented itself in the form of an equation of differences, and which had impeded my progress several years since in attempting the solution of a problem connected with the game of chess.

XXXVI. *Notices respecting New Books.*

Appendix to Euclid's Elements: containing original Propositions in Geometry, designed for the young Student as Exercises under the various Propositions in Euclid's Elements and Data. By I. Newton: printed for the Author by N. Walker, Wisbech, 1825.

RESPECTING the merit of Euclid's production we have had many different opinions:—some considering it defective in definition, arrangement and demonstration; while others contend that it forms the most perfect code of elementary instruction that has ever appeared; and were we allowed to give our opinion on the subject, we should say with the latter, that the work has no equal.—But although Euclid has done much as an elementary writer, he has not done all; a wide field is open for others to traverse, and accordingly we find that in this as well as in the last age, geometers have attempted both to enlarge and improve the labours of the Grecian sage. Among these we again find the name of Isaac Newton. This gentleman has for some time been a respectable contributor to the different periodical publications of the day. The number of valuable and curious questions he has proposed, and the neat and elegant solutions he has given, indicate how well his mind is fitted for those studies which one of his name (whose stupendous imagination comprehended the world) pursued with the most splendid success.

The publication of Mr. Newton professes to be an Appendix to Euclid's Elements. The author in his preface judiciously remarks, that in order to become a geometer, it is not enough merely to read the writings of the illustrious Greek; something more is wanted to rouse the curiosity of the student and stimulate him to active exertion. For this purpose Mr. Newton has drawn up a series of choice propositions, and arranged them

them so as to form valuable exercises under the correspondent propositions in the Elements. The execution of this little work meets our approbation, and we hope much good will be derived from its perusal. We regret, however, to find that Mr. N. in framing some of the theorems does not express himself in definite language—Take for example, prop. 11, book 1st. "In one of the sides of a given triangle, to find a point that shall be equidistant from two given points in the base." We confess that under the present form of the proposition we do not see how this is to be done, that is, generally; and we fear that many of Mr. Newton's readers will find themselves similarly situated. We have however no doubt but Mr. Newton thought, although he has not written, mathematically; and to show him how we are puzzled by his Proposition, we shall take a particular example of our own; and we expect that our failure in attempting to solve it will induce Mr. Newton to remove this and similar defects in the next edition.

Suppose the two sides of a triangle to be the one 24 yards, the other 27, and the base 30. Now let the given points in the base be, the one distant from one extremity 10 yards, and the other 25, making an interval between the points of 15 yards.

Find a point in the shorter side that shall be equidistant from both the given points.

The propositions in the Data are possessed of considerable merit; and those entitled "promiscuous" cannot fail to be highly useful to such as can find time and have a taste for geometrical pursuits. The practical examples under the head "Trigonometry" form an acquisition to the work of no small consequence; and their being accompanied by the table of sines and tangents required in the solution, puts it in the reader's power actually to determine what is required.

We cannot conclude, without again expressing our high approbation of this little performance taken as a whole, and earnestly recommend it to every geometrical student as a collection of valuable exercises. Y. X.

A Letter to the Editors of the Philosophical Magazine and Journal, upon the Correspondence between Sir JAMES EDWARD SMITH, *and Mr.* LINDLEY, *which has lately appeared in that Journal. By* JOHN LINDLEY, *Esq. F.L.S. &c.* Ridgway and Sons.

We learn from this pamphlet, addressed to ourselves, that its appearance has been occasioned by our having decided against a further continuation in our Journal of the controversy alluded to, in declining to insert a second reply from Mr. Lindley; which he has now therefore published in a separate

form, together with the correspondence already printed in the Philosophical Magazine, and accompanied by some animadversions on the soundness of our judgement in the performance of our editorial duty in the present case.

The Editors of this Journal have more than once had occasion to deprecate the uncourteous style and acrimonious feeling too often, and most unnecessarily, introduced into scientific controversy; and in several instances they have, as in the present case, judged it right to refuse to allow those pages to be occupied with angry personalities, which should be devoted to the advancement of knowledge. The present Editor is fully disposed to pursue the same course, as most agreeable to himself and most advantageous to the cause of science.

In the present instance the Editor must rest his justification entirely on the tone and temper of Mr. Lindley's letters: he is quite at a loss to find anything in the first letter of Sir J. E. Smith that could call for the style which Mr. Lindley has unfortunately adopted, and against which, with the most friendly feelings towards that gentleman, he thought it right to protest. The first letter of Sir J. E. Smith, instead of being intended as an attack on Mr. Lindley, originated, as the Editor is fully persuaded, in the kindness of the writer towards himself, and a wish to assist the Philosophical Magazine by an occasional contribution.

Mr. Lindley alleges that he has only "imitated the controversial manner peculiar to Sir J. E. Smith"! Here the Editor is completely at issue with him; and concludes by confidently appealing to the entire difference of their "controversial manner" as his justification for declining to insert the reply, wholly disavowing those partial or prudential motives which are suggested in this pamphlet.

Recently published.

The Second Part of the Philosophical Transactions of the Royal Society for 1824, has just appeared, and the following are its contents:

Some curious Facts respecting the Walrus and Seal, discovered by the Examination of Specimens brought to England by the different Ships lately returned from the Polar Circle. By Sir Everard Home, Bart. V. P.R.S. In a Letter addressed to Sir Humphry Davy, Bart. Pres. R.S.—Additional Experiments and Observations on the Application of Electrical Combinations to the Preservation of the Copper Sheathing of Ships, and to other purposes. By Sir Humphry Davy, Bart. Pres. R.S.*—On the Apparent Direction of Eyes in a Portrait.

* Given in the present Number, p. 203.

trait. By William Hyde Wollaston, M.D. and V.P.R.S.—Further particulars of a case of Pneumato-thorax. By John Davy, M.D. F.R.S.—On the Action of finely divided Platinum on Gaseous Mixtures, and its Application to their Analysis. By William Henry, M.D. F.R.S.—A Comparison of Barometrical Measurement, with the Trigonometrical Determination of a Height at Spitzbergen. By Captain Edward Sabine, of the Royal Regiment of Artillery, F.R.S.—Experimental Inquiries relative to the Distribution and Changes of the Magnetic Intensity in Ships of War. By George Harvey, Esq. Communicated by John Barrow, Esq. F.R.S—Experiments on the Elasticity and Strength of hard and soft Steel. In a Letter to Thomas Young, M.D. For. Sec. R.S. By Mr. Thomas Tredgold, Civil Engineer. Communicated by Dr. Young.—A short Account of some Observations made with Chronometers in two Expeditions sent out by the Admiralty, at the recommendation of the Board of Longitude, for ascertaining the Longitude of Madeira and of Falmouth. In a Letter to Thomas Young, M.D. For. Sec. R.S. and Secretary to the Board of Longitude. By Dr. John Lewis Tiarks. Communicated by Dr. Young.—Of the Effects of the Density of Air on the Rates of Chronometers. By George Harvey, F.R.S.E. Communicated by Davies Gilbert, Esq. V.P.R.S.—A Letter from Lewis Weston Dillwyn, Esq. F.R.S. addressed to Sir Humphry Davy, Bart. P.R.S.—An Account of the Organs of Generation of the Mexican Proeteus, called by the natives Axolotl. By Sir Everard Home, Bart. V.P.R.S.—An Account of Experiments on the Velocity of Sound, made in Holland. By. Dr. G. Moll, Professor of Natural Philosophy in the University of Utrecht, and Dr. A. Van Beek. Communicated by Capt. H. Kater, F.R.S.—A Catalogue of nearly all the principal fixed Stars between the Zenith of Cape Town, Cape of Good Hope, and the South Pole, reduced to the 1st of January 1824. By the Rev. Fearon Fallows, M.A. F.R.S.—Remarks on the Parallax of α Lyræ. By J. Brinkley, D.D. F.R.S. Andrew's Professor of Astronomy in the University of Dublin.—Appendix:—List of Presents:—Meteorological Journal.

Icones Fossilium sectiles, auctore C. E. König, F.R. & L.S. &c. &c. Sowerby, No. 156, Regent-street.

Corso Analitico di Chimica. Analytical Course of Chemistry, by M. Mojon. Gênes, 1825.

Chimica applicata all' Agricoltura. M. Chaptal's Agricultural Chemistry, translated by M. Primo. Milan, 1824.

Caroli Linnæi Philosophia Botanica; editio aucta et emendata. Tournay, 1824.

Traité

Traité élémentaire de Physique; par C. Despretz. Paris, 1824.

Coup d'œil sur les Mines; par L. Elie de Beaumont, Ingenieur des Mines. Paris, 1824.

In the Press.

The Mine Laws of Mexico, which are but little known in this country, are now translating from the last regulated code, in the original Spanish, and will shortly be ready; with Observations on Mines generally, as also the Mining Associations.

XXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 3.—THE readings of Dr. Williams's paper On the maternal-fœtal circulation was resumed and concluded: and Dr. J. R. Johnson, F.R.S. communicated some further observations on the genus *Planaria.*

March 10.—J. F. W. Herschel, Esq. Sec. R.S. communicated a paper entitled Improvements on Leslie's Photometer; by W. Ritchie, A.M.

March 17.—The Society for promoting Animal Chemistry communicated a paper by Sir E. Home, Bart. V.P.R.S. entitled Observations on the influence of the nerves and ganglions in producing animal heat.

March 24.—A paper was read, entitled Results of meteorological observations taken at the Madras observatory; by John Goldingham, Esq. F.R.S.: and the Society then adjourned over two Thursdays, to meet again on the 14th of April.

LINNÆAN SOCIETY.

March 1 and 15. — The reading of Messrs. Sheppard and Whitear's paper On the birds of Norfolk and Suffolk, and of Dr. Hamilton's Commentary on the *Hortus Malabaricus,* was continued.

GEOLOGICAL SOCIETY.

Feb. 18.—A paper by Professor Buckland was read, On the Valley of Kingsclere, near Newbury, and the evidence it affords of disturbances affecting the green sand, chalk, and plastic clay formations.

The object of this paper is to describe the phænomena of a small valley near Kingsclere, in which the green sand strata are protruded to the surface through the chalk and plastic clay,

clay, at a spot situated within the area of the chalk basin of Newbury, and affording a remarkable exception to the general regularity of that basin.

This irregularity of structure has apparently originated from a sudden elevation of the chalk, accompanied by fracture and an inverted dip; its position is remarkable as being near Inkpen Hill, a point where the chalk rises to 1011 feet, the highest elevation it attains in England.

In the valley subjacent to the Inkpen ridge, and near its north base, the chalk dips rapidly in two opposite directions, nearly north and south, on each side of a central axis or anticlinal line; and a little further east the green sand also emerges with a similar double dip, and forms the small valley of Kingsclere, surrounded on all sides with an inclosing escarpment of chalk.

The north frontier of this valley is in close contact with well characterized deposits of plastic clay dipping like itself rapidly towards the north. Four similar valleys are adduced in the counties of Wilts and Dorset; and the author concludes respecting them all, that it is utterly impossible to explain their origin by denudation alone, nor indeed without referring the present position of their component strata to a force acting from below and elevating the strata along the line of the central axis of the valleys in question. To valleys of this kind the author applies the appellation of Valleys of Elevation, to distinguish them from those which owe their origin simply to diluvial denudation. He then proceeds to show that the valleys of Pewsey near Devizes, and of the Wily and the Nadder above Salisbury, have also to a certain degree been affected by a force acting from beneath and elevating the strata at a period antecedent to their being submitted to denudation; and concludes that not only these inclosed valleys similar to that of Kingsclere, but many open valleys also (though in all cases modified by subsequent denudation), had a prior origin, arising from the fracture and elevation of their component strata. This must have happened in the case of the weald of Kent and Sussex, inclosed as it is with an escarpment of chalk dipping every where outwards in opposite directions, and sometimes very rapidly along the North and South Downs.

The author proceeds to illustrate, by the position of the strata of plastic clay in the same district, the important geological question,—whether the chalk was disposed in its present form of troughs or basins before or after the deposition of the tertiary formations now inclosed in them; and to show that the present inclination of the strata along the south frontier of the basins of London and Hants took place since the deposition of

of the plastic, and probably also of the London clays; and that these two basins were once connected together across the now intermediate chalky strata of the downs of Hants, Wilts, and Dorset; since it appears that the plastic clay formation is so far from being limited to the lower levels of the present basins, that large residuary fragments of it still occur on the summits of the most elevated portions of chalk in these counties, *e. g.* on the summit of Inkpen near Newbury, and on that of Blackdown near Abbotsbury, as well as on the top of Chidbury and Beacon hills in the highest part of Salisbury plain. The strata that covered the intermediate spaces have probably been removed by diluvial denudation, and the destructible nature of their component materials would render them peculiarly liable to be swept away by the transit of violent currents of water. The wreck of the harder portions of the sandy strata thus destroyed forms the sandstone blocks called Grey Weathers that lie loosely scattered on the naked surface of the chalk in all these counties, and of which Stonehenge is constructed. In lower levels within the existing basins these same strata have been less destroyed, in consequence of the greater protection their low position has afforded them from the ravages of diluvial denudation.

The author concludes with referring to the occurrence of similar tertiary strata, as well as of chalk and green sand, on the summits of the Savoy Alps, nearly 10,000 feet above the level of the sea, where they seem to bear the same relation to the tertiary strata of the valleys of Italy, France, and Germany, that our trifling elevations of Inkpen, Blackdown, &c. bear to the basins of London and Hants; and concludes that since the depositions of these beds, either by the elevation of the mountains or the depression of the valleys, or the united effect of both these causes, the relative level of the one to the other has been changed to the amount of many thousand feet.

March 4.—A notice was read On some silicified wood from the desert between Cairo and Suez, in a letter from George Francis Grey, Esq. to the Rev. W. Buckland, Pres. G.S.

Large masses of silicified wood, resembling in form the trunks of palm-trees, lie scattered, the author observes, over a tract of gravel in the desert, about 15 miles from Cairo, and for two days' journey all the way from that place to Suez.

A notice was also read On the bones of several animals found in peat near Romsey in Hampshire, extracted from a letter from Charles Daman, Esq. to the Rev. W. Buckland, P. G. S.

Mr. Daman mentions that the skulls of several beavers, as well as the bones of oxen, swine, stags and roe-bucks, have been dug out of the peat near Romsey and out of the shell marl

marl provincially termed "malen," which occurs in the same alluvial tract. In one place several human skeletons have been taken out of the marl.

A paper entitled, Observations on the beds of clay, sand and gravel, belonging to the red marl formation of the midland counties, and on the rocks from which they are derived, by the Rev. James Yates, M.G.S., was read in part.

ASTRONOMICAL SOCIETY.

March 11.—There was read "An account of the arrival and erection of Fraunhofer's large Refracting Telescope at the Observatory of the Imperial University at Dorpat:" communicated in a letter from Prof. Struve to Francis Baily, Esq. President. Prof. Struve received this telescope in November last, and was happy to find that although it had travelled more than 300 German miles, its several parts had been so carefully packed that none of them had sustained the slightest injury. When in a perpendicular position, the height of the object-glass is 16 feet 4 inches (Paris measure) from the floor, 13 feet 7 inches of which belong to the telescope itself; so that the eye-glass stands 2 feet 7 inches from the floor. The diameter of the object-glass is 9 Paris inches (about 9¾ inches English). The weight of the whole instrument is about 3000 Russian pounds. It is so constructed that it may be used as an equatorial. The upper part of the instrument consists of the tube, with its axis of motion, two graduated circles, and a variety of levers and counterpoises, producing the most perfect equilibrium in every direction, and providing against all friction. The declination circle is divided from 10′ to 10′, but by means of the vernier may be read off to 5″. The instrument may be turned in declination with the finger, and round the polar axis with still less force.

The most perfect motion round the polar axis is produced by means of clock-work, which is the principal feature of this instrument, and the greatest triumph for the artist, the mechanism being as simple as it is ingenious. A weight, attached to a projection connected with the endless screw, overcomes the friction of the machine. The clock vibrating in a circle regulates the motion by moving an endless screw connected with a second wheel in the above projection. The weight of the clock as well as that of the friction may be wound up without the motion being interrupted. When the telescope is thus kept in motion, the star will remain quietly in the centre, even when magnified 700 times. At the same time there is not the least shake or wavering of the tube, and it seems as if we were observing an immoveable sky.

 But

But the artist has done still more; he has introduced a hand on a graduated dial of the clock, by which the motion of the latter can be instantly altered; so that a star may be brought to any point of the field of vision to which it may suit the observer to carry it, according as it is required to make the course of the instrument go faster or slower than the motion of the heavens; and if once placed, it may be kept in that position by returning the hand to its original position. The same mechanism is also used to make the motion of the instrument coincide with that of the sun and moon.

This instrument has four eye-glasses, the least of which magnifies 175 times, and the largest 700 times.

M. Struve has compared the power of this telescope with Shroeter's 25-feet reflector, by means of which that astronomer saw σ *Orionis* twelve or thirteen fold; whereas Struve clearly ascertained the existence of sixteen distinct stars.

This instrument is furnished with four annular micrometers of Fraunhofer's construction, and an excellent net-micrometer of the same artist. By means of these it appears that the probable error in the measurement of some minute distances, of 7″ and under, did not exceed the 18th part of a second. The expense of this instrument was about 950*l.* sterling.

There was also read a paper on "A new Zenith Micrometer;" by Charles Babbage, Esq. F.R.S. &c. The object of the inventor in this instrument is to supersede the necessity of extreme accuracy in the divisions. The principle on which this instrument depends may be readily comprehended by imagining a parallelogram, admitting of free motion about its four angles, to be placed with two of its sides in a horizontal position, and the whole in a vertical plane; and a telescope to be fixed at right angles to the lower horizontal bar of this parallelogram. Here every motion of one of the perpendicular bars of the instrument round its upper joint will not change the angle which the telescope makes with the meridian, but will merely remove it into a new position, in which it will point to the same object in the heavens. But if either of the horizontal bars of the instrument be lengthened by a very small quantity, this parallelism of the telescope will no longer be preserved; but any movement of the upright bars round their axes will not only remove the telescope from its position, but will cause it to form a very small angle with its former direction. The magnitude of that angle will depend on the alteration in the length of the arm of the parallelogram, and also on the angle which that arm makes with its first direction. The minutiæ of the construction depend upon these considerations, but cannot be rendered intelligible without a diagram.

The

The arc which is actually measured in the heavens by means of this instrument is determined by a formula, in which the sum of three arcs is taken from the semicircumference; one of them resulting from the actual observation, the other two from a cosine and a tangent, ascertainable by computation from the theorem itself. In an extensive use of this micrometer, tables may easily be formed to facilitate the computation.

ROYAL IRISH ACADEMY.

On Thursday, the 16th of March, the annual elections at the Royal Irish Academy took place, when the several official departments were filled up as follow:—*President:* Rev. John Brinkley, D.D. F.R.S. &c.—*Vice-Presidents:* Joseph Clarke, M.D.; Colonel Edward Hill; the Provost of Trinity College, Dublin; Wm. Brooke, M.D.—*Treasurer:* Wm. Brooke, M.D. —*Secretaries:* Rev. J. H. Singer, D.D. F.T.C.D.; Rev. F. Sadleir, D.D. S.F.T.C.D.—*Secretary of Foreign Correspondence:* Colonel E. Hill.—*Librarian:* Rev. W. H. Drummond, D.D.—*Committee of Science:* The Archbishop of Dublin; Joseph Clarke, M.D.; the Provost; Rev. F. Sadleir, D.D. S.F.T.C.D.; Sir C. L. Giesecke; Rev. R. MacDonnell, D.D. F.T.C.D.; Rev. Dionysius Lardner, A.M.—*Committee of Polite Literature:* Rev. J. H. Singer, D.D. F.T.C.D.; Andr. Carmichael, Esq.; Sam. Litton, M.D.; Rev. C. R. Elrington, D.D. F.T.C.D.; Rev. W. H. Drummond, D.D.; Geo. Kiernan, Esq.; M.W. Hartstonge, Esq.—*Committee of Antiquities:* Colonel E. Hill; Wm. Brooke, M.D.; Isaac D'Olier, LL.D.; Rev. H. H. Harte, F.T.C.D.; Tho. H. Orpen, M.D.; Hugh Ferguson, M.D.; Sir F. L. Blosse, Bart.

MEDICAL SOCIETY OF LONDON.

The fifty-second Anniversary Meeting of this Society was held on Tuesday, the 8th of March, at the London Coffee-house, Ludgate-hill, William Shearman, M.D. President, in the chair. The Officers and Council for the year ensuing are:

President: Henry Clutterbuck, M.D.—*Vice-Presidents:* H. J. Cholmly, M.D.; James Johnson, M.D.; Sir Astley Paston Cooper, Bart. F.R.S.; and William Kingdon, Esq. —*Treasurer:* John Andree, Esq.—*Librarian:* David Uwins, M.D.—*Secretaries:* T. J. Pettigrew, Esq. F.A.S. F.L.S.; and Thomas Callaway, Esq.—*Foreign Secretary:* Leonard Stewart, M.D.—*Council:* Thomas Walshman, M.D.; William Shearman, M.D.; George Darling, M.D.; Thomas Cox, M.D.; John Russell, M.D.; J. B. James, M.D. F.L.S.; Edward Morton, M.D.; George Drysdale; Edward Sutcliffe; Burton

 Brown;

Brown; James Dunlap; William Lake; Kennedy Johnson; Samuel Ashwell; E. A. Lloyd; James Handey; Edward Leese; Henry Edwards; W. D. Cordell; Joseph Amesbury; William Burrows; Septimus Wray; H. B. C. Hillier; Montague Gosset; T. W. Chevalier; George Langstaff; J. C. Taunton; Henry Hensleigh; J. M. Mugglestone; J. S. Smith; R. W. Bampfield; Robert Brien; Robert Blicke; and Martin Ware, Esquires.

To deliver the anniversary oration in March 1826, John Haslam, M.D.—*Registrar:* James Field, Esq.

Mr. E. A. Lloyd delivered the annual oration; the subject was the "Constitutional Treatment of Organic Diseases." The Fellows and their friends then adjourned to dinner in the great room of the tavern. The Ex-President, William Shearman, M.D., as usual, taking the chair. The number present was ninety.

In conformity with the will of the late Dr. Anthony Fothergill, the Society offers the Annual Gold Medal, value twenty guineas, for the best dissertation on a subject proposed by them, for which prize the learned of all countries are invited as candidates.

The subject for this year is "The Nature and Treatment of Carcinoma."

1. Each dissertation must be delivered to the registrar, in the Latin or English language, on or before the first of December.

2. With it must be delivered a sealed packet with some motto or device on the outside, and within, the author's name and designation, that the Society may know how to address the successful candidate.

3. No paper in the handwriting of the author, or with his name affixed, can be received; and if the author of any paper shall directly or indirectly discover himself to the Committee of papers, or any member thereof, such paper will be excluded from all competion for the medal.

4. The prize medal will be presented to the successful candidate, or his substitute, at the Anniversary Meeting of the Society in March 1826.

5. All the dissertations, the successful one excepted, will be returned, if desired, with the sealed packet unopened.

*** The subject of the dissertation for the year 1826–7 is "Contagion and Infection."

MEDICO-BOTANICAL SOCIETY.

At a meeting of the Medico-Botanical Society of London, holden the 11th of March the following particulars of the

New

New Essential Oil of Laurus, (written by Dr. Hancock of Demerara, and communicated to the Society by Lieut. Friend, R.N. F.R.S.) were read.—

"The knowledge of this oil has hitherto been almost exclusively confined to the natives of Spanish Guiana. This substance, which has been very injudiciously termed *Azeyte de Sassafras*, (an appellation which tends to confound it with the essential oil yielded by the *Laurus Sassafras* of the northern continent of America,) affords, so far as my knowledge extends, an extraordinary and solitary instance of the production of a perfectly volatile liquid without the aid of art. Substituting for the appellation to which I have objected the provisional name 'Native Oil of Laurel,' I shall describe the method of procuring it, and enumerate its principal chemical and medicinal properties, so far as they have been investigated and examined. The native oil is yielded by a tree of considerable magnitude; its wood is aromatic, compact in its texture, of a brownish colour, and its root abounds with essential oil. This tree, which is found in the vast forests which cover the flat and fertile regions between the Oronooko and the Panine, has from an analogy already alluded to been supposed to belong to the natural order *Lauri*; and though Humboldt and Bonpland do not seem to have been acquainted with its singular and important produce, its botanical characters may very possibly have been described in their *Plantæ Equinoctiales* under the genera *Ocotea*, *Persea*, or *Litsea*. This question, however, I am unable to solve, as I have never seen the parts of fructification.

"The native oil of laurel is produced by striking with an axe the proper vessel in the internal layers of the bark, while a calabash is held to receive the fluid. So obscure, however, are the indications of these reservoirs, that the Indians (with perhaps a little of their usual exaggeration) assert that a person unacquainted with the art may hew down a hundred trees without collecting a drop of the precious fluid. In many of its properties the native oil resembles the essential oil obtained by distillation and other artificial processes: it is, however, more volatile and highly rectified than any of them, its specific gravity hardly exceeding that of alcohol. When pure it is colourless and transparent; its taste is warm and pungent; its odour aromatic, and closely allied to that of the oily and resinous juice of the *Coniferæ*:—so striking is this resemblance, that a friend to whose inspection I submitted the oil pronounced it rather hastily to be spirits of turpentine. It is volatile, and evaporates without residuum at the atmospheric temperature. It is inflammable, burning entirely away, and except

except when mixed with alcohol gives out in its combustion a dense smoke. Neither the alkalies nor acids seem to exert any sensible action on the native oil. Upon dropping into it sulphuric acid, the latter assumes a momentary brownish tinge, but soon regains its transparency, remaining immiscible at the bottom of the vessel. The oil of laurel dissolves camphor, caoutchouc, wax, and resins, and readily combines with the volatile and fixed oils. It is insoluble in water; soluble in alcohol and æther. Though the specific gravity of the oil exceeds that of æther, yet the compound formed by combining them in the proportion of one part of the former to two of the latter, floats upon the surface of pure æther, and may therefore be the lightest of all known liquids.

"With respect to the medicinal properties of the native oil, it bears when externally applied the character of a powerful discutient, and appears when exhibited internally to be a diaphoretic, diuretic, and resolvent. By many it is believed to be analeptic, alterative, anodyne, and to promote the exfoliation of carious bones. Without listening to the extravagant reports of the Indians, who exalt it into a panacea, we must admit that its efficacy has been demonstrated in cases of rheumatism, swellings of the joints, cold tremours, pains in the limbs, and in various disorders supposed to originate in a vitiated state of the blood (*mala sangre*).

"In all these cases it is exhibited in doses of 20 to 24 drops on sugar twice a day, accompanied by frequent and long continued friction of the parts affected with the oil; while the body is kept moderately warm and a free use of diluting drinks prescribed to the patient. The same practice is said to have been attended with the happiest effect in paralytic disorders: for this I cannot vouch; but have found it a valuable remedy in cases of nervous and rheumatic head-ache, sprains, and bruises. A decoction of the root has been employed as an alterative in various chronic complaints, and with much success. I am fully aware of the re-action that often results from over-excited and disappointed expectation, and the discredit into which a new remedy frequently falls in consequence of the unmerited encomiums which those who bring it into notice have injudicioully bestowed upon its virtues:

Quicquid excessit modum
Pendet instabili loco.

"However slight the credibility we may feel inclined to attach to the evidence of the Indians, (upon which our knowledge of the medicinal properties of the native oil almost entirely reposes,) the information derived from experience surely claims that attention, and justly challenges that examination which

which we should not hesitate to bestow upon the speculations of the mere theorist. Let inquiries be instituted and experiments be made by those who by situation and scientific attainments are qualified for the task. By these investigations it may not only be ascertained what degree of confidence ought to be reposed in the unqualified encomiums which the Indians lavish upon this anomalous production, but properties unknown to them may be discovered, and its history, which they have been accused (perhaps unjustly) of involving in obscurity, be satisfactorily elucidated.

"To the chemist and the vegetable physiologist in particular, the native oil of laurel, elaborated by the unassisted hand of Nature in a state of purity which the operose processes of Art may equal but cannot surpass, presents an interesting subject for inquiry and a wide field for speculation."

ROYAL ACADEMY OF SCIENCES OF PARIS.

Dec. 13.—A sealed note relative to a new experiment was deposited with the secretary by M. Fresnel.—M. Jules Cloquet read a memoir on the effects and mode of effecting acupuncturation.—M. Bascary, in the artillery service, presented two memoirs on perspective: the first on a portable instrument, called a *Coordonometer*, designed to draw exactly after nature the perspective of any plane or spot whatsoever; the second, on the adoption and utility of exact perspective: with applications to military drawing. (Messrs. de Prony and Fresnel, commissioners.)—The Academy proceeded to a scrutiny of votes for the election of a member of the botanical section, to fill up the vacancy left by M. Thouin. Out of 52 votes, M. le Vicomte de Morel de Vindé obtained 46 suffrages; M. Aug. Saint-Hilaire 3; M. Emmanuel d'Harcourt 1; M. Michaux 1: M. Morel de Vindé was declared elected.—M. de Férussac read a memoir On the geography of the *Mollusca*. The sections of Agriculture and Botany presented, as candidates for the Chair of culture and naturalization of exotic plants at the Museum of Natural History, MM. de Mirbel and Bosc.

Dec. 20.—M. Geoffroy Saint-Hilaire presented two of his memoirs; one entitled The composition of the osseous head of man and of animals, extracted from the *Annales des Sciences Naturelles;* the other, an article extracted from the eleventh volume of the *Mémoires du Muséum d'Histoire Naturelle*, is entitled, Of the opercular and auricular fins of fishes, considered as a principal point on which should turn every research respecting the determination of the parts or pieces which compose the cranium of animals, followed by synoptical tables, exhibiting the number and explaining the composition of these parts.—

parts.—M. Desmoulins, who had previously read to the Academy, on the 31st May last, a memoir on the differences existing between the nervous system of the lamprey and that of the vertebrated animals, presented to the Academy the result of new observations which he has been making at Rouen, since the commencement of December, on two other species of *Petromyzon.*—M. Magendie read a memoir On a liquid which is found in the cavity of the vertebral canal, and in a portion of that of the cranium in man, as also in a portion of that of the mammiferous animals in general. The Academy proceeded to the election for the above-named professorship at the *Museum d'Histoire Naturelle.* The number of votes were 53. M. Mirbel had 28; M. Bosc 24; and M. Saint-Hilaire 1. The procès-verbal of this election will be addressed to the Minister of the Interior.

Dec. 27.—M. the Keeper of the Seals, invited the Academy to nominate one of its members to form part of the commission for reporting upon the types of the royal printing office. M. Lacroix was appointed to this service. — M. Mathieu was named, instead of M. Cauchy, member of the commission for examining the papers of M. Peyrard.—M. Delise de Vire read the remainder of his History of Lichens: this was transmitted to the commissioners already named, MM. Desfontaines and Bosc.—M. Clapeyron addressed, on the part of an author who had not made himself known, the description of a machine for the prize of M. de Monthyon.—An anonymous memoir On apoplexy, for the prize of M. de Monthyon, was also deposited by one of the secretaries.—M. Arago presented a memoir On the action exercised by copper, with respect to the oscillations of the magnetic needle. (MM. Poisson, Ampère and Dulong, commissioners.)—M. Vauquelin made a verbal report upon the Dictionary of Chemistry of M. Pelletan junior.—M. Magendie communicated, verbally, some new details relative to the liquid contained in the cranium and vertebral canal. He opened, at the hospital de la Charité, the body of a man recently dead. The vertebral canal was entirely filled with liquid. This liquid surrounds the anterior nerves of the interiors; it equally separates the several fibres, both of the nerves of sensation and those of motion. It appears to be more abundant in man than in animals.—M. Poisson read a second memoir On the theory of magnetism.—M. Flourens read a memoir On the brain of fishes. This he connected with the subjects of two other memoirs: the first On the cicatrization of wounds of the brain, and the re-production of the integumental parts; the second, On the fundamental condition of the hearing, and on the several causes of deafness. (MM. Cuvier, de Humboldt, Portal, Duméril and Dulong, commissioners).—

missioners).—M. Cauchy presented two memoirs, On the integration of linear equations, and On their application to various problems in physics.

XXXVIII. *Intelligence and Miscellaneous Articles.*

MR. BARLOW'S MAGNETICAL DISCOVERIES.

THE emperor of Russia has presented Mr. Barlow, of the Royal Military Academy, (through his excellency Count Lieven,) with a valuable gold watch and rich dress chain, as a mark of the value which his majesty places upon the magnetical discoveries of that gentleman, and on their important application to the improvement and security of navigation. We are glad also to add that the East India Board has followed the example of the Admiralty and Trinity Boards, and presented Mr. Barlow with the sum of two hundred pounds. Mr. Barlow not having availed himself of a patent right for his correcting plate, is justly entitled to these marks of public acknowledgement.

IMPORTANT CORRECTION RELATIVE TO THE CURE OF GLANDERS.

We have been informed on good authority that the fit mode of exhibition of the sulphate of copper, as a remedy for the contagious glanders of horses, (noticed in Phil. Mag. for September 1824,) is in a *liquid* state, and not as usual in a ball: it is supposed the form of a *ball* occasions the medicine to remain so long in the stomach as to produce mischief from irritation.

USE OF CROTON TIGLIUM IN VETERINARY MEDICINE.

It is well known that horses do not easily undergo the operation of purging, and scarcely any cathartic which readily affects the human bowels operates on the brute creature. Aloes alone, or along with calomel, purges the horse. Oil, indeed, is also slightly laxative: but of late an accession has been made by the oil of Croton Tiglium, revived by Mr. Short. This, in the dose of 15 to 25 drops, works as effectually as aloes: but the latest improvement is the exhibition of the dried seeds, or even the husk of the dried seeds after the oil has been expressed, 20 to 30 grains of which answers as well as the oil of Croton.

The danger from an over-dose is as great from this new drug as from aloes. For a small and weakly horse the dose should be less than above stated. Five grains of the Croton seeds is calculated to be equal to about one drachm of aloes for

for producing nausea; and of course six times this quantity as a purge, is equal to six drachms of aloes.

A single grain of the Croton Tiglium seeds, or even half a grain, is a powerful purgative to the human creature. Rumphius of old speaks of 4 grains of the seeds as a poison used by "wicked wives to get rid of their husbands."

SIEMEN'S IMPROVEMENT ON THE PROCESS OF MAKING BRANDY FROM POTATOES.

The introduction of this process, which has been adopted in many parts of Germany and in the north of Europe, has been recommended to the Swedish government by M. Berzelius, and to the Danish government by Professor Oersted. From the trials made at Copenhagen, it would appear that one-third more brandy is produced than by the usual processes. In Professor Oersted's report, we find the following account of the process:—The potatoes are put into a close wooden vessel, and exposed to the action of steam, which heats them more than boiling water. The potatoes can thus be reduced to the state of the finest paste with the greatest facility, it being necessary only to stir them with an iron instrument furnished with cross pieces. Boiling water is then added to the paste, and afterwards a little potash, rendered caustic by quicklime. This dissolves the vegetable albumen which opposes the complete conversion of the potato starch into a fluid. Professor Oersted frees the potato brandy from its peculiar flavour by means of the chlorate of potash, which is said to make it equal to the best brandy made from wine.—*Gill's Tech. Repos.*

NATURE OF WHITE PRECIPITATE.

In some inquiries connected with the preparation of calomel upon the large scale conducted in the laboratories at Apothecaries' Hall, Mr. Hennell has discovered several curious and important facts respecting the chlorides of mercury, more especially in relation to the triple compounds formed by corrosive sublimate with other chlorides. He has ascertained that certain chlorides which appear to have no action upon calomel at common temperatures, decompose it at a boiling heat, to a greater or less extent, and resolve it into corrosive sublimate and metallic mercury.

"I had repeatedly noticed," he observes, "a bluish tint which calomel acquires when washed in a boiling solution of muriate of ammonia, as directed by the London Pharmacopœia, to remove corrosive sublimate. To ascertain the cause, I boiled 100 grains of pure calomel in a solution of muriate

muriate of ammonia, containing 100 grains of the salt. The change of colour in a few minutes was very evident. The solution, when tested, contained corrosive sublimate. The boiling was continued with four other portions of muriate of ammonia, 100 grains each; when the calomel was entirely decomposed, 40 grains of mercury remained. Sixty grains of white precipitate were obtained from the solutions by carbonate of soda. There was no decomposition of the sal ammoniac. With common salt I obtained the same results, mercury remaining, and white precipitate being thrown down from the solutions, by liquid ammonia. Common salt is not so active in producing these changes; as *ten* portions of 100 grains each were used before the decomposition of 100 grains of calomel was complete. Muriate of potass and the earthy muriates have, I have every reason to believe, the same power; but I did not push the experiments as in the case of soda and ammonia."

The action of chlorides upon calomel Mr. Hennell has particularly investigated in respect to common salt and muriate of ammonia, those being the substances usually employed for the purpose of washing calomel, under the idea of freeing it from corrosive sublimate, an effect which they fulfil when employed cold and in dilute solution only. But when perfectly pure calomel is boiled for a few minutes in a solution of muriate of ammonia or of common salt, and a portion of the liquor filtered off and tested, a portion of sublimate is always found in it; and on boiling for a long time, the whole of the calomel is decomposed, and compounds of sal ammoniac and corrosive sublimate, and of common salt and corrosive sublimate, are obtained, an equivalent portion of metallic mercury being at the same time separated.

These facts are peculiarly important in relation to the preparation of calomel, inasmuch as the Pharmacopœia directs the use of a hot solution of muriate of ammonia, with the intention of freeing it from any accidental admixture of corrosive sublimate; and Dr. Henry, in describing the methods of ascertaining the purity of calomel, directs it to be boiled in solution of muriate of ammonia. "When carbonate of potassa," he observes, "is added to the filtered solution, no precipitation will ensue, if the calomel be pure*." Several other chemists of eminence have given this as a criterion by which to recognise the presence of corrosive sublimate in calomel; whereas it appears from Mr. Hennell's experiments, that the protochloride of mercury is in such cases decomposed, and that perchloride is formed.

* Elements of Experimental Chemistry, 9th edition, p. 588.

Having inferred from previous experiments that the "white precipitate" was a compound of one proportional of peroxide of mercury and one of muriate of ammonia, Mr. Hennell verified his opinion as follows. A solution of one proportional of corrosive sublimate (=272) was mixed with a quantity of solution of ammonia, containing two proportionals ($17 \times 2 = 34$) of that alkali; a neutral mixture resulted, white precipitate was formed, and one proportional of muriate of ammonia (ammonia 17 + muriatic acid 37 = 54 of muriate of ammonia) was found in solution. In this case, the 2 proportionals of chlorine in the sublimate ($36 \times 2 = 72$) were converted, at the expense of 2 proportionals of water, into 2 of muriatic acid, which, uniting with the ammonia, formed 2 of muriate of ammonia. The 2 proportionals of the oxygen from the water (equivalent to the 2 of hydrogen transferred to the chlorine) united to the 1 proportional of mercury in the sublimate, to form 1 of peroxide of mercury, which fell in combination with 1 of muriate of ammonia to constitute white precipitate; while the other proportional of muriate remained as above stated in solution. The equivalent number, therefore, of white precipitate, is 270, and it consists of

1 proportional of peroxide of mercury =	216	80
1 ———— muriate of ammonia =	54	20
	270	100

Having thus synthetically established the composition of white precipitate, the following analytical experiment was made upon it: 270 grains were dissolved in hydrocyanic acid, and sulphuretted hydrogen was passed through the solution till it occasioned no further change; the precipitate was then collected, washed, and dried: it weighed very nearly 232 grains, being the equivalent of bisulphuret of mercury. The filtered liquor, on evaporation to dryness, left 54 grains, or 1 proportional of muriate of ammonia.—*Journal of Science*, Jan. 1825.

ELECTRICAL CONDUCTING POWER OF MELTED RESINOUS BODIES.

It is commonly stated that melted resins become good conductors of electricity, and freely allow of its transmission. The following experiments were made with the view of determining to what extent they possessed this property.

Common resin, shell-lac, asphaltum, bees-wax, red and black sealing-wax, were melted in separate glass tubes, fitted with wires for taking the electric spark: they all slowly and with difficulty drew off the charge of a jar, and not with the facility usually supposed. The melted contents of the same tubes acted as non-conductors when made part of the Voltaic circuit.

Several

Several thin glass tubes (previously tried by metallic coatings) were coated outside with copper foil, and about half-filled with the melted substances, having wires dipping into them, similar to small Leyden vials. The resinous coating, however, distributed no charge over the interior of the glass tubes when connected with the machine, which would have been the case with conductors.

Upon removing the copper coatings and wires, substituting pointed wires bent at right angles, resting against the interior of the glass tube beneath the melted bodies, and suspending them successively from an electrified conductor, placing a metallic rod outside opposite the points, sparks passed in all cases perforating the glass.

The last cases would indicate that melted resinous bodies are not conductors, and the results obtained in the first instance may possibly be referred to heated air about the apparatus. T. G.—*Journal of Science*, Jan. 1825.

MOTION OF THE ELECTRIC FLUID.

It has long been received as a fact, that an electrical discharge was capable of being transmitted through a very considerable distance (say three or four miles) instantaneously, and without any sensible diminution of its intensity. Mr. Barlow, however, by employing wires of various lengths, up to 840 feet, and measuring the energy of the electric action by the deflection produced in a magnetic needle, has found that the intensity diminishes very rapidly, and very nearly as the inverse square of the distance. Hence the idea of constructing electrical telegraphs is quite chimerical. He found, also, that the effect was greater with a wire of a certain size, than with one smaller, yet that nothing was gained by increasing the diameter of the wire beyond a given limit.

SURVEY OF THE PERSIAN GULF.

The surveying vessels, Discovery and Psyche, will leave Bombay about the end of the month to continue the survey of the Persian Gulf; the examination of which has been completed from Ras Moosendem, at the entrance, to the island of Bahrein. Until the year 1821, the coast, with the exception of a small portion containing the pirate ports, was comparatively unknown. In the vicinity of the Cape it is high, rugged, and intersected by deep estuaries, the two largest of which have been named after the present governor of Bombay, and commander-in-chief, Elphinstone's Inlet, and Colville's Cove. It was this part that obtained from the ancients the denomination of Asabo, or Black Mountains; without doubt from the colour of the rocks, which are principally composed of

of black basalt and clinkstone, with calcareous spar in veins. Some occurrences of the columniated basalt were observed, but the general arrangement was in the form of mountain caps, as they are termed by mineralogists. Several of the small valleys were in a high state of cultivation; the soil being formed from the debris of the basalt, which is well known to afford one of the richest composts for vegetation. The inhabitants appeared a mixed race between the Bedouins and Muscat Arabs. The mountainous part of the coast terminates at Raumps, between which and the harbour of Abothubbe, are situated the pirate ports. From the last-mentioned place, to the westward, comprising 200 miles in longitude, and 150 in latitude, the coast had hitherto never been explored by Europeans. Here were discovered numerous islands; between a long chain of which, connected by extensive reefs, and the main, is an inlet, forty miles deep, navigable for the largest vessels, and sheltered from the prevailing heavy winds. The main land is formed in some parts of low sandy ground, and in others of hills, which are evidently of volcanic origin. The islands discovered by Capt. Maude have been surveyed, and distinct plans made of each; stronger marks are here evinced of volcanic influence; sulphur and its combinations are found in all; the hills are conical, and contain volcanic scoriæ, intermixed with argillaceous earth; gypsum, in most of its varieties; a recent formation of trap; most of the ores of iron, and obsidian. In all parts of the gulf, particularly on the Persian shore, traces of a similar nature are found sufficient to denote its being what geologists would term a volcanic country, and which will readily account for the late earthquake in that quarter. The survey in June last terminated at the interesting island of Bahrein; the topography of which is unknown, with the exception of a small part in the vicinity of the city. The whole line of coast was laid down by a continued series of triangles, and the principal positions were verified by celestial observations; between the two extremes it forms an irregular curve, comprising, with the various sinuosities, upwards of a thousand miles. The space between Bahrein and the mouth of the Euphrates will be completed by the close of the next cool season, unless any extraordinary difficulties should present themselves.—*Bombay Gazette, Sept.* 22, 1824.

M. VAUQUELIN ON THE PRESENCE OF TITANIUM IN MICA.

We extract from the Annals of Philosophy for March 1825, the following notice by M. Vauquelin, on this subject, which gave rise to the memoir by M. Peschier, given in our present Number, with a note appended to it by Mr. Children.

"M. Vauquelin,

"M. Vauquelin, at the request of Mr. Peschier of Geneva (who conceived that he had found titanium in several micas in such quantity as to be an essential constituent of the mineral), repeated his experiments, first on two varieties of mica, and afterwards on many others, in all of which he detected the presence of titanium, but in very minute quantity, and in different proportions; the richest in titanium did not give more than one per cent. of that metal.

"His mode of analysis was as follows:—He ignited the mica (divided into thin laminæ, and cut very small with a pair of scissars), with two parts of caustic potash, for half an hour, and digested the mass in 100 parts of water. Muriatic acid was gradually added to the mixture till it was slightly in excess; the solution evaporated slowly to dryness; the residuum washed with cold water, and the silica separated by the filter.

"If the silica was coloured, which often happened, he digested it in cold muriatic acid diluted with 10 parts of water, till it became white; it was then washed, and while still moist boiled in strong muriatic acid. The liquid was then evaporated to expel the greater part of the acid, diluted and filtered; and the solution, containing only a slight excess of acid, treated with an infusion of galls. If titanium was present the solution first assumed a yellowish-red colour, and soon afterwards tannate of titanium separated in flakes of the same colour.

"The muriate of titanium is so easily decomposed by heat, that in general the greater part of the metal is found with the silica, which should always be carefully examined in all analyses in which titanium may be expected to be discovered. If, on the other hand, the evaporation have not been carried far enough, a portion may remain in solution in the washings of the silica. To be certain, precipitate the solution by ammonia, wash the precipitate, and digest it in caustic potash, which will dissolve the alumina and the oxide of titanium, and the latter may then be separated by saturating the alkali with muriatic acid, and precipitation by infusion of galls.

"Nearly two years since, I examined a dark brown mica, from Siberia, for titanium, without finding the least trace of that metal."

PNEUMATIC MECHANISM ENABLING THE WALRUS TO CARRY ON PROGRESSIVE MOTION AGAINST GRAVITY.

Sir Everard Home's papers containing his discoveries of the pneumatic mechanism enabling the fly and the gecko to carry on progressive motion against gravity are already well known to our readers. The following is his account of an analogous

analogous provision in the walrus, given in a paper on some curious facts respecting that animal and the seal, published in the Phil. Trans. for 1824, part ii.

"The first discovery I shall mention," says Sir Everard, "is a peculiarity in the structure of the hind flipper or foot of the walrus, that has not been adverted to; nor could it have been done now by any one not well acquainted with the mechanism of the foot of the fly, enabling it to support its weight, and carry on progressive motion against gravity. Such is the general resemblance between this flipper and the foot of the fly, that having upon a former occasion seen it in a very mutilated state, macerating in water, I discovered this analogy, and requested my friend Captain Sabine in the Artillery, at the time he sailed with Captain Clavering to make experiments on the figure of the earth, to bring me the feet and other parts of the walrus. With the assistance and exertions of Mr. Rowland, assistant-surgeon to the ship, he has complied with my request, and enabled me to bring forward the following observations on this subject.

"It is a curious circumstance that two animals, so different in size, should have feet so similar in their use. In the fly, the parts require being magnified one hundred times to render this structure distinctly visible; and in the walrus, the parts are so large as to require being reduced four diameters to bring them within the size of a quarto page. As a knowledge of the structure of the fly's foot led to the detection of the use of the hind flipper of the walrus, so, on the other hand, an examination of the toes of the walrus has enabled me to make out the use of a part of the foot of the fly which I did not sufficiently understand—I mean the two points: Mr. Adams called them pickers, from supposing that they entered certain small holes in the surface on which progressive motion was carried on. This opinion I did not deem worthy of consideration, but was unable to make out their real use: on comparing them, however, with the outer toes of the walrus, they are evidently intended to surround the exhausted cavity, so that a vacuum may be more suddenly and perfectly formed.

"As the skin of the animal is very thick and unyielding, and had been for so long a time in strong brine, the parts were much shrunk and corrugated; but even in this state they showed that the palm of the flipper formed a concavity, which had the appearance of a cup when the great and little toes were made to encircle the others. In this state of the parts this concavity was thrown into longitudinal rugæ, so that the real size could not be ascertained, the span from the point of the great toe to the end of the little toe not exceeding twelve inches. After the

the thick skin thrown into rugæ upon the palm was dissected off, the flipper lost all appearance of a foot, and took on that of the hand of a giant, so far as respected the bones and muscles, differing indeed in having a web covering all the other parts, and extending beyond the point of the thumb and fingers. The span, instead of being twelve inches, became twenty-eight. The resemblance of the bones of the hind flipper of a walrus to those of the human hand, (which I believe is like nothing else in nature,) is curiously exact: the bones of the wrist are the same in number and shape; so are those of the metacarpus; so also the phalanges of the thumb and fingers. The tendons of the perforantes muscles pass through those of the perforati in the palm upon the metacarpal bones, while in the human hand this takes place upon the first phalanges of the fingers; and there are no lumbricales muscles whatever. On the back of this gigantic hand I was astonished to find the tendon of the indicator muscle.

"The muscles and tendons that are peculiar to this flipper, not met with in the human hand, are those of the web which extends beyond the fingers and thumb: this web is a strong ligamentous elastic substance intermixed with muscular fibres; it has a set of muscles, which have their origin from the sides of the last phalanges of the fingers insensibly lost in it, and tendons go off from each side of the perforator muscles, which spread out and are lost in it.

"That this gigantic hand is employed as a cupping-glass to prevent the animal from falling back in its movements, whether on the ice or in climbing the rocky cliffs, there can be no doubt; for it is only necessary to take the human hand, and envelope it in an elastic web extending some way beyond the points of the fingers, to prove that it could perform such an office: but when we find the lumbricales muscles wanting, the only use of which is to clench the fist, it adds to the proof; and when the indicator is met with, a mode of opening a valve to let the air in is pointed out.

"It may be doubted whether the extent of the flippers is equal to the support of the enormous bulk of this animal; but this doubt will be removed when I mention that Mr. Fisher informs me that a walrus killed at Spitzbergen weighed twenty hundred weight, and that an exhausted surface of twenty-eight inches by twenty will support a pressure of 15lbs. on every square inch, more than double the animal's weight.

"That the principle on which the foot of the fly, the gecko, and the walrus is formed, is the same, I trust has been established. In the fly there are two cups, in the walrus only one."

 Results

Results of the Meteorological Tables at the end of the Philosophical Magazine, from the 25th of December 1823 to the 25th of December 1824.

By William Burney, *LL.D.*

1824.	Gosport, at half-past 8 o'clock A.M. Barometer in Inches, &c.	Thermometer.	Temperature of Sp. Water.	Hygrometer.	Evaporation in Inches, &c.	Rain in Inches, &c.	London. Barometer at 1 P.M.	London. Thermometer at 8 A.M.	Boston. Barometer at 8½ A.M.	Boston. Thermometer at 8½ A.M.	London. Rain in Inches, &c.	Boston. Rain in Inches, &c.
	In.	°	°	°	In.	In.	In.	°	In.	°	In.	In.
January ...	29·956	38·52	50·40	77·1	0·72	2·105	29·986	37·68	29·766	36·68	1·05	0·97
February ...	29·820	41·19	49·12	75·4	0·98	2·715	29·852	38·42	29·678	38·06	1·83	1·19
March	29·770	41·17	48·79	71·7	1·50	3·805	29·810	38·79	29·621	39·29	1·38	2·46
April........	29·880	43·29	48·12	62·2	2·47	2·282	29·915	40·09	29·717	41·53	1·50	1·69
May	29·861	52·40	48·27	62·4	2·41	4·631	29·893	48·10	29·624	50·05	3·00	2·40
June	29·965	59·13	49·27	54·8	4·58	3·085	29·986	52·35	29·671	56·06	1·50	3·46
July	30·040	64·90	50·54	54·2	5·66	1·385	30·046	60·97	29·552	63·95	3·90	1·26
August.......	29·934	62·39	51·73	61·1	4·28	5·015	29·948	59·16	29·500	60·84	2·60	2·01
September..	29·951	62·64	52·86	65·7	3·75	2·690	29·988	59·45	29·557	60·29	3·50	3·33
October.....	29·645	52·30	53·45	69·5	3·03	4·136	29·689	48·63	29·404	49·45	4·42	2·82
November...	29·688	50·22	53·04	71·7	2·30	4·325	29·689	45·58	29·381	44·61	2·67	3·82
December...	29·737	44·43	52·27	73·9	1·10	4·490	29·759	41·57	29·480	40·48	2·90	2·92
Averages.	29·854	51·05	50·65	66·6	32·78	40·664	29·880	47·56	29·579	48·44	30·25	28·33

From the annual mean pressure at these places, I infer that Mr. Cary's barometer is placed 30 feet above the low-water mark of the river Thames, and that Mr. Veall's is 200 feet above the low-water mark of the river Witham, mine being 50 feet above the low-water mark of Portsmouth harbour. This inference is made upon the supposition that the mercury in each of our barometers is of the same specific gravity, and similarly affected in its contractions and expansions by equal pressures of the atmospherical column. I suspect, however, that 200 feet is a great deal higher than Mr. Veall's barometer is placed above the low-water mark of the river Witham; and that 30 feet is lower than Mr. Cary's is placed above the low-water mark of the Thames; especially as the tide rises 19 feet in the latter river.

I am and have long been of opinion that correct barometers will never be procured by our meteorologists in different parts of the country, till the cause is taken up by such scientific men as those who form the Meteorological Society, and the manufacturing of them confined to experienced artists under their superintendence: for some use barometers made by one artist, and others by other artists; and it is doubtful whether all those who register their observations confine themselves to the portable upright barometers, which are the most simple in construction, and far more correct and equal in the scale of their movements by equal pressures, than the wheel-barometers can be, from the friction to which they are liable, and the *unequal proportion of their pulleys to the circumference of the scales.* I have seen wheel-barometers of an elegant appearance at the time of a very low pressure, (as that on the 24th of December 1821,) below the range of their scales of from 28 to 31 inches; the index of one was $\frac{2}{10}$ths, of another $\frac{3}{10}$ths, and of a third nearly $\frac{5}{10}$ths of an inch below the bottom of the scales; while the mercury in the upright barometers in the same neighbourhood and at the same height did not sink below 28·10 inches—the lowest I ever observed it in mine. I trust the members of the Meteorological Society will not deem my suggestion obtrusive, as they are well aware of the great discrepancies in the indications of barometers at different places and at the same height from the level of the sea, and of the perplexity they occasion to observers in making their comparisons even with mean annual pressures. The next circumstance that claims my attention in the foregoing table is the temperature of the external air. It appears that the mean of Mr. Cary's thermometer this year, at 8 o'clock A.M., is about $3\frac{1}{2}$ degrees lower than the mean of mine at half-past 8 A.M. From a knowledge of the annual mean temperature

of the external air in the interior of London, the mean temperature here last year, and the difference between the mean at 8 and half-past 8 o'clock A.M., the mean of Mr. Cary's thermometer, as given in the table at 8 A.M., appears to be about one degree too low: I should therefore like to know in what situation he placed his external thermometer, or whether he registered from the one attached to his barometer within-doors.

The annual average temperature of Mr. Veall's thermometer is about $2\frac{3}{5}$ degrees lower than mine at half-past 8 A.M., which is as it should be, considering the greater north latitude of Boston, and the localities of that place and Gosport in respect to their contiguity to the sea, which has been found by long experience to modify the chilly air in winter, and keep down the excessive heat in summer.

The difference between the mean temperature of the external air here this year, by a good horizontal self-registering day and night thermometer, and the mean at half-past 8 o'clock A.M. by a Fahrenheit's, is $\frac{24}{25}$ths of a degree: the mean temperature of the external air at Boston for 1824 may therefore be taken at $49^\circ\frac{2}{3}$, that is $2^\circ\frac{3}{5}$ less than the mean of this place. And for the last eight years of Mr. Veall's registering, the mean temperature of the air at Boston may be taken at $49^\circ\frac{1}{2}$, that is one degree below the mean temperature of London for a series of years.

Lastly, it may be seen by the table that the annual amount of rain caught at Gosport differs considerably from either that caught in London or at Boston. Gosport being on the south-west side of Portsmouth harbour, at the extremity of the southern coast of Hampshire, it therefore receives the full force of the W. and S.W. winds from the Atlantic Ocean, which, on an average of ten years' observations made here three times every 24 hours, prevail nearly four months out of the year, according to the following scale of the winds for that purpose.

A Scale of the Winds for 10 *Years, ending with* 1824.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
$326\frac{1}{4}$	$391\frac{1}{2}$	$370\frac{1}{2}$	$305\frac{1}{4}$	$353\frac{1}{4}$	$649\frac{1}{2}$	$716\frac{1}{4}$	$540\frac{1}{2}$	3653

It further appears from this scale, that by rejecting the winds from the North and South points, which are almost equal in duration to each other, the winds from the West side of our meridian have prevailed $1906\frac{1}{4}$ days, and only $1067\frac{1}{4}$ from

from the East side, which is as 2 to 1 nearly: this difference is probably induced in some measure by a contiguous and extensive sea, over which the air is free and unobstructed towards the land.

Causes of the great Difference in the yearly Amount of Rain caught at different Places in the British Isles.

For a proof of the facts, I will select five places in which the annual mean depth of rain, for a series of years, has been determined by punctual observations, viz.

Gosport.	London.	Boston.	New Malton.	Kendal.
In.	In.	In.	In.	In.
34·50	25·20	24	40	60

I observed before that we have the full force of the W. and S.W. winds here; and it may be relied on that before the rain that is brought up by the prevailing westerly winds arrives in London, it is not only diminished in quantity by precipitation, but the passing *nimbi*, or rain-clouds, are altered in their electrical state, by inosculating with the warmer land air: this alteration, with the comparative dryness of the land over the sea air, and the artificial heat of populous places over which the rain-clouds pass, tend to absorb and hold in solution a great portion of the passing vapours that would be otherwise condensed and fall in rain in London or at Boston. Upon this principle of reasoning, a less quantity of rain falls at Boston than in London, which we see by the table is the case; because the tract of land over which it is moved by the prevailing westerly winds is of greater extent from the shores of South Wales to Boston than it is from the southern shore of England to London; consequently about one-fourth of the rain is spent in its passage over the land before it arrives at these places.

With respect to the difference of rain in places of a higher North latitude:—Taking the annual depth at Kendal, in Westmoreland, to be one-third more than it is at New Malton, in Yorkshire, which is very near the truth; to account for this great difference in places too that are almost upon the same parallel of latitude, it is necessary in the first place to ascertain the character of the prevailing winds in both places, which I find by the meteorological tables to be at least *one-fourth* of the year from the South-west point, from which quarter the British Isles receive most of the rain, as it comes immediately across the Western Ocean.

The mean yearly depth of rain at Kendal is 60 inches, and that

that at New Malton 40 inches. By looking at the map of the United Kingdom, it will be seen that the South-west winds in their passage to Kendal must go directly up St. George's Channel into the Irish Sea; and in this direction, after arriving at the Land's End, they collect additional vapours, which readily yield to condensation between the English and Irish shores, and thus produce the additional depth of rain at Kendal, in comparison of that at New Malton and places situated more towards the southern shores of Britain. Moreover, as Kendal is situated at the southern extremity of the mountainous district of Westmoreland and Cumberland, the passing vapours are attracted by the mountains, and the attraction is an additional means of augmenting the annual depth of rain there. The prevailing South-west winds, in their passage to New Malton, first strike upon Harleigh, in North Wales, thence they proceed over Chester, Manchester, &c., a distance of between three and four degrees over the land, in which distance the bulk of rain is considerably diminished. By these means I account for the great difference in the yearly depth of rain at Kendal and New Malton; and am of opinion that the same causes hold good in respect to the difference in other places in the country.

LIST OF NEW PATENTS.

To John Heathcoat, of Tiverton, lace-manufacturer, for an improved method of producing figures or ornaments on goods manufactured from silk, cotton, &c.—Dated 25th February 1825.—6 months to enrol specification.

To David Edwards, of King-street, Bloomsbury, writing-desk manufacturer, for an ink-stand in which by pressure the ink is caused to flow to use.—26th February.—2 months.

To Joseph Manton, of Hanover-square, gun-maker, for improvements in fire-arms.—26th February.—6 months.

To William Hopkins Hill, of Woolwich, lieutenant of artillery, for improvements in machinery for propelling vessels.—26th February —6 months.

To George Augustus Kollmann, of the Friary, Saint James's Place, Middlesex, professor of music, for improvements in the mechanism and construction of piano-fortes.—26th February.—2 months.

To James Bateman, of Upper-street, Islington, for a portable life-boat. 26th February.—2 months.

To Cornelius Whitehouse, of Wednesbury, whitesmith, for improvements in manufacturing tubes for gas, &c.—26th February.—6 months.

To Thomas Attwood, of Birmingham, for an improved method of making nibs or slots in copper or other metal cylinders used for printing cottons, &c.—26th February.—6 months.

To David Gordon, of Basinghall-street, and William Bowser, of Parsons-street, Wellclose-square, iron-manufacturer, for improvements in plating or coating iron with copper, &c.—26th February.—6 mo.

To Chevalier Joseph de Mettemberg, of Foley-place, Mary-le-bone, physician, for a vegetable mercurial and spirituous preparation called *Quintessence Antipsorique*, or Mellemberg's Water, and also a particular method of employing the same by absorption as a specific and cosmetic.—26th of February.—6 months.

To

To John Masterman, of No. 68, Old Broad-street, for an improved method of corking bottles.—5th March,—6 months.

To Abraham Howry Chambers, and Ennis Chambers, of Stratford-place, Mary-le-bone, and Charles Jearrard, of Adam-street, Manchester-square, for a new filtering apparatus.—5th March.—6 months.

To William Halley, of Holland-street, Blackfriars Road, Surrey, iron-founder and blowing-machine maker, for improvements in forges, and on bellows or apparatus to be used therewith or separate.—5th March.—4 months.

To Robert Winch, of Steward's Buildings, Battersea Fields, Surrey, engineer, for improvements in rotary pumps for raising water, &c.—5th March.—6 months.

To William Henry James, of Cobourg-place, Winson Green, near Birmingham, engineer, for improvements on rail-ways, and carriages to be employed thereon.—5th March.—6 months.

To William Hirst and John Wood, both of Leeds, for improvements in cleaning, milling, or fulling cloth.—5th March.—6 months.

To John Linnell Bond, of Newman-street, Mary-le-bone, architect, and James Turner, of Well-street, Mary-le-bone, builder, for improvements in the construction of windows, casements, folding sashes (usually called French sashes), and doors, by means of which the same are hung and hinged in a manner adapted more effectually to exclude rain and wind and to afford a free circulation of air.—9th March.—2 months.

To Thomas Hancock, of Goswel Mews, St. Luke's, Middlesex, patent cork-manufacturer, for a new manufacture which may be used as a substitute for leather and otherwise.—15th March.—6 months.

To Thomas Hancock, of Goswel Mews, for improvements in making ships' bottoms, vessels and utensils of different descriptions and various manufactures, and porous or fibrous substances, impervious to air and water, and for coating and protecting the furnaces of different metallic and other bodies.—15th March.—6 months.

To Thomas Hancock, of Goswel Mews, for improvements in the process of making or manufacturing ropes or cordage and other articles from hemp, flax, &c.—15th March.—6 months.

To John Colling, of Lambeth, engineer, for improvements on springs and other apparatus used for closing doors.—15th March.—6 months.

To Robert Bretell Bate, of the Poultry, optician, for his improvement on the frames of eye-glasses.—15th March.—6 months.

To Henry Nunn, and George Freeman, both of Blackfriars Road, Surrey, lace-manufacturers, for improvements in machinery for making that sort of lace commonly known by the name of bobbin-net.—15th March.—6 mon.

To Samuel Brown, of Saville-row, Middlesex, commander in the royal navy, for his apparatus for giving motion to vessels employed in inland navigation.—15th March.—4 months.

To Joseph Barlow, of the New Road, St. George's, Middlesex, sugar-refiner, for his process for bleaching and clarifying and improving the quality and colour of sugars known by the name of bastard and piece sugars.—15th March.—6 months.

To William Grisinthwaite, of King's Place, Nottingham, for his improvement in air-engines.—15th March.—6 months.

To Richard Whitechurch and John Whitechurch, of Star-yard, Cary-street, Middlesex, for an improvement on hinges for doors, &c. which will enable the doors, &c. to be opened on the right and left (changing the hinges), and with or without a rising hinge.—17th March.—2 months.

To Mark Cosnahan, of the Isle of Man, for a new apparatus for ascertaining the way and leeway of ships, also applicable to other useful purposes.—17th March.—6 months.

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr. Burney at Gosport, Mr. Cary in London, and Mr. Veall at Boston.*

Days of Month, 1845.	Gosport, at half-past Eight o'Clock, A.M. Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Clouds. Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Height of Barometer, in Inches, &c. Lond. 1 P.M.	Bost. 8½ A.M.	Thermometer. London. 8 A.M.	Noon.	11 P.M.	Boston. 8½ A.M.	Rain. London.	Boston.	Weather. London.	Boston.	Wind.
☽ Feb. 26	30·20	40	49·40	67	E.	...	0·310	...	...	...	...	...	1	1	30·40	30·15	35	37	37	35·0	...	...	Cloudy	Snow	Calm
27	29·64	44	......	83	S.	...	·105	...	...	1	...	...	1	1	29·67	29·70	35	44	40	35	...	0·08	Rain	Rain	SE.
28	29·48	40	......	71	E.	0·11	......	1	1	1	...	1	1	...	29·60	29·45	36	42	40	31·5	...	·03	Cloudy	Fine	Calm
March 1	29·68	40	49·10	76	E.	...	·800	...	...	1	...	...	1	1	29·62	29·60	34	46	39	33·5	...	...	Cloudy	Fine, rain at night	S.
2	29·20	43	......	76	W.	...	·360	...	...	1	...	...	1	1	29·27	29·02	40	46	40	35·5	...	·62	Cloudy	Rain, do.	W.
3	29·38	39	......	74	W.	·15	·085	1	...	1	...	1	1	1	29·48	29·22	34	44	35	34·5	0·70	·17	Fair	Fine, hail *p.m.*	SW.
○ 4	29·54	40	......	77	N.	...	......	...	...	1	...	...	1	1	29·80	29·50	34	42	35	35	...	·05	Cloudy	Fine	E.
5	30·18	34	......	72	N.	...	·080	...	...	1	..	...	1	1	30·30	30	33	42	36	34	...	...	Cloudy	Fine	W.
6	30·08	43	......	70	SW.	·10	·710	...	...	1	...	...	1	1	30·11	30	37	44	40	35·5	...	...	Cloudy	Fine, rain at night	S.
7	29·57	43	49·00	78	S.	...	·020	1	...	1	...	...	1	1	29·70	29·55	40	46	44	37	...	·32	Cloudy	Rain	SE.
8	30·14	40	......	77	N.	...	·045	1	...	1	...	...	...	1	30·20	30·06	40	48	44	35·0	...	·23	Fair	Fine	Calm
9	30·22	52	......	74	W.	·20	......	...	...	1	...	...	1	...	30·18	30	44	50	50	47·0	...	...	Cloudy	Cloudy	S.
10	30·22	52	......	77	W.	...	·040	...	...	1	...	...	...	1	30·30	29·95	50	51	50	51	0·20	...	Cloudy	Cloudy	S.
11	30·14	50	......	79	SW.	...	......	...	...	1	...	...	...	1	30·14	29·75	47	52	47	51	...	·30	Rain	Rain, rain *a.m.* early	S.
12	30·13	45	......	74	NW.	·10	......	1	...	1	...	1	1	...	30·24	29·90	44	50	42	45	...	...	Fair	Fine	W.
13	30·10	46	......	72	NW.	...	·185	...	...	1	...	...	1	1	30·13	29·95	43	45	46	43·5	...	...	Rain	Cloudy	Calm
14	29·96	38	49·00	71	NE.	...	......	1	...	...	...	1	1	...	30·14	30	33	36	34	34·5	...	...	Cloudy	Snow	SE.
15	30·07	36	......	64	NE.	·25	......	...	...	...	...	1	1	...	30·25	30·05	33	36	32	35·0	...	·3	Cloudy	Cloudy	E.
16	30·19	34	......	63	N.	...	......	1	...	1	...	1	1	...	30·26	30·10	34	37	32	34·5	...	...	Fair	Snow	W.
17	30·34	33	......	62	E.	...	......	1	1	1	...	1	...	...	30·40	30·33	28	36	31	32	0·10	·2	Fair	Fine	E.
18	30·47	37	......	57	SE.	·25	......	1	...	1	...	1	...	...	30·58	30·40	32	44	32	32·5	...	...	Fair	Fine	E.
19	30·53	38	......	62	E.	...	......	1	1	1	...	...	...	...	30·67	30·45	32	45	37	36	...	...	Fair	Fine	SE.
20	30·56	38	......	66	N.	...	......	1	...	1	...	...	...	...	30·65	30·50	35	50	40	34·5	...	...	Fair	Fine	W.
21	30·51	41	48·95	63	NE.	·45	......	1	1	1	...	1	...	...	30·56	30·47	36	47	40	36·5	...	...	Fair	Cloudy	Calm
22	30·26	42	......	67	NE.	...	......	...	...	...	...	...	1	1	30·32	30·17	36	40	38	40	...	...	Cloudy	Cloudy	Calm
23	30·04	41	......	60	NE.	...	......	...	...	...	...	1	...	...	30·17	30·10	40	46	35	41·5	...	...	Fair	Fine	E.
24	29·97	40	......	63	NE.	...	......	1	...	...	...	1	...	...	30·03	30·06	34	46	38	38	0·09	...	Fair	Fine	E.
25	29·78	41	48·90	60	SE.	·45	·175	1	1	1	...	...	1	1	29·85	29·65	40	49	39	39	...	...	Fair	Cloudy	E.
Aver.:	30·021	41·07	49·06	69·8		2 06	2·915	14	5	22	0	11	18	15	[illegible]·10	29 92	37	44	[illegible]	37·6	1·09	1·85			

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

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XXXIX. *On the Theory of the Figure of the Earth.* By J. Ivory, *Esq. M.A. F.R.S.*

"CE cas est jusqu'à présent le seul pour lequel on ait trouvé une solution rigoreuse, qu'on doit à Maclaurin; de sorte que le problème de la figure de la terre, envisagé physiquement, n'est résolu exactement qu'en supposant le sphéroide fluid et homogène."—*Lagrange, Méc. Anal.* tom. i. p. 204.

These are the words of the correct and elegant Lagrange, after having demonstrated that a homogeneous elliptical spheroid fulfills the conditions of equilibrium when it revolves upon an axis and its particles attract one another in the inverse proportion, of the square of the distance. They must not be understood in the utmost extent of their meaning; for what is proved, falls short of a physical solution of the problem of the figure of the earth, even on the supposition of a homogeneous fluid. Two things are necessary to a complete theory:—in the first place, we must know the laws of the equilibrium of a homogeneous mass of fluid, the particles of which attract one another and are subjected to a centrifugal force; in the second place, we must investigate all the figures that possess the properties which these laws require. Maclaurin merely proved that a homogeneous fluid, which has the figure of an oblate spheroid, will be *in equilibrio* when its particles are acted upon by the supposed forces.

After much discussion and many attempts, the conditions of the equilibrium of a fluid mass, such as we find them in all the books at the present day, were given by Clairaut in his work on the figure of the earth.

"Clairaut publia en 1743 son ouvrage sur la Théorie de la Figure de la Terre. Il y donne les equations générales jusqu'alors inconnués, de l'équilibre des fluides soit homogènes, soit heterogènes, ou composés d'un nombre quelconque de fluides, quelles que soient les forces qui animent chacune de leurs molecules, et en supposant entre ces molecules une attraction mutuelle suivant une loi quelconque."—*Méc. Céleste*, liv. 11me, p. 6. 1823.

 The

The result of Clairaut's researches are thus concisely stated in another passage of the same author:

"Les conditions de l'équilibre d'une masse fluide sont donc: 1°, que la direction de la pesanteur soit perpendiculaire à chaque point de la surface extérieure; 2°, que dans l'intérieur de la masse, les directions de la pesanteur de chaque molecule, soient perpendiculaire à la surface des couches de densité constante. Comme on peut dans l'intérieur d'une masse homogène, prendre telles couches que l'on veut, pour couches de densitè constante;—la seconde des deux conditions précédentes de l'équilibre est toujours satisfaite, et il suffit pour l'équilibre que la première soit remplie; c'est-à-dire que la resultante de toutes les forces qui animent chaque molecule de la surface extérieure, soit perpendiculaire a cette surface." —*Méc. Céleste*, tom. 2de, liv. 3^{me}, p. 64.

Confining our attention to the case of a homogeneous fluid, the conditions of equilibrium are extremely simple. The outer surface must be defined by an equation between three independent co-ordinates; and the resultant of all the forces acting at every point of the same surface must be perpendicular to it. Being in possession of a theory so little complicated, we should expect to be able to deduce from it the beautiful discovery of Maclaurin; and to prove, *à priori*, that the elliptical spheroid is one figure of equilibrium, although, perhaps, not the only one. No geometer has succeeded in this investigation. It may be said with truth, that the received theory of the equilibrium of fluids is of little use in the question of the figure of the planets; for the small change induced upon a fluid sphere by the action of a centrifugal force is a mathematical consequence of the perpendicularity of gravity to the outer surface.

In a paper inserted in the Philosophical Transactions for 1824, I have shown that there is an inadvertency in the theory of equilibrium as it is delivered by Clairaut, which is the true cause of all the difficulty and embarrassment that has occurred in the determination of the figure of the planets. The conditions of equilibrium are rightly assigned when there is no mutual attraction between the particles; or, to speak more precisely, when all the forces that act upon a particle are independent of the matter without the level surface in which the particle is placed. When the particles are endowed with attractive powers, Clairaut's theory is insufficient; and it becomes necessary to add a new condition in order to ensure the equilibrium. In the case of a homogeneous fluid composed of particles that mutually attract one another, the equilibrium requires, 1st, That the resultant of all the forces acting

ing upon a particle in the outer surface be perpendicular to that surface; 2dly, That any interior body of the fluid bounded by a level surface be *in equilibrio* with respect to the attraction of all the exterior matter. The latter condition cannot be fulfilled unless every level stratum possess such a figure as to attract all particles in the inside with equal force in opposite directions.

In the paper alluded to, the subject is examined at great length. The inadvertence is pointed out both in the original investigation of Clairaut, and in the usual theory of fluids, founded on the principle of equal pressure in all directions. It is proved that both these methods lead to the conditions of equilibrium above mentioned, when no part of the forces in action is left out. The case of a homogeneous fluid revolving upon an axis, and composed of particles which attract one another inversely as the square of the distance, is next considered; and the conditions of equilibrium are investigated by means of particular considerations without the aid of any general theory.—Thinking that it may gratify some of the readers of the Philosophical Magazine, to whom the progress of the philosophy of Newton is not altogether indifferent, I have subjoined the last-mentioned investigation, which does not require algebraical calculations. It is contained in the two following propositions.

Prop. I.—If a homogeneous fluid body revolving upon an axis be *in equilibrio* by the attraction of its particles in the inverse proportion of the square of the distance; any other mass of the same fluid having a similar figure, and revolving upon an axis similarly placed, will likewise be *in equilibrio*, supposing that its particles attract one another by the same law.

Suppose that the two bodies are similarly divided into the same indefinitely great number of molecules, of which dm and dm' are any two situated alike, and therefore having their volumes and quantities of matter proportional to the volumes and quantities of matter of the two whole bodies. Take also any two points similarly situated, either within both bodies or in their outer surfaces; and let f and f' denote the respective distances of the two points from the molecules dm and dm', and r and r' their distances from the two axes of revolution.

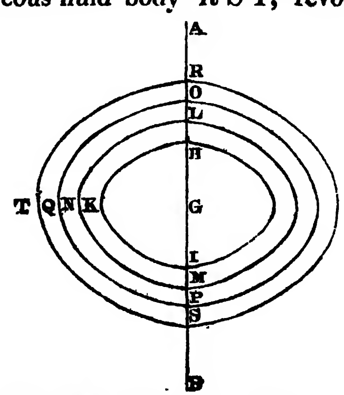
The forces with which the molecules dm and dm' attract two equal particles of matter placed in the assumed points are porportional to $\frac{dm}{f^2}$ and $\frac{dm'}{f'^2}$; and, in these fractions, the numerators being proportional to the cubes, and the denominators to the squares of any two homologous lines of the re-

 spective

spective bodies, the attractive forces will be simply proportional to any two such lines. The lines f and f', in the directions of which the forces act, are likewise similarly situated within the two bodies. And as what has just been proved is true of the attractions of any two molecules similarly situated in the two bodies, it follows that the resultants of all the attractive forces acting upon the two points will be to one another in the proportion of any homologous lines of the respective bodies, and that they will act in similar directions. Again: since the velocity of rotation is the same in both bodies, the centrifugal forces urging the particles placed in the two assumed points, will be proportional to the respective distances from the axes of revolution; that is, to r and r', or to any homologous lines of the respective bodies; and, as the same forces act in the prolongations of r and r', they will have similar directions. Wherefore, since any two particles similarly placed on the surfaces, or within the respective bodies, are urged in like directions and with proportional intensities by the attractive and centrifugal forces that act upon them, it is easy to prove that the pressures propagated by these forces in the two bodies will be entirely similar, and will always have the same proportion to one another. Wherefore the equilibrium of one body is a necessary consequence of the equilibrium of the other body.

PROP. II.—If a homogeneous mass of fluid revolve about its axis, and be *in equilibrio* by the attraction of its particles in the inverse proportion of the square of the distance, all the level surfaces will be similar to the outer one; and any stratum of the fluid contained between two level surfaces will attract particles in the inside with equal force in opposite directions.

Suppose that the homogeneous fluid body R S T, revolving about the axis A B, is *in equilibrio* by the centrifugal force and the attraction of its particles in the inverse proportion of the square of the distance:—the axis of rotation A B will pass through G, the centre of gravity of the fluid mass. In the interior of the revolving body, trace round the point G the surfaces Q O P, N L M, K H I, similar and similarly situated to the outer surface, indefinitely near one another, and



intercepting

intercepting very thin strata of the fluid between them. Then the whole fluid body R S T, and the part of it bounded by the surface O P Q, are similar to one another in their figure; and they revolve about the common axis A B, which cuts them both similarly; wherefore, because the first body is *in equilibrio*, the latter will also be *in equilibrio*, supposing that it revolves by itself, the exterior stratum being taken away or annihilated *. And, because the body O P Q is *in equilibrio* when it revolves by itself, the gravitation at its surface, or the resultant of the attraction of its particles and the centrifugal force, will at every point be perpendicular to that surface. Wherefore every particle of the fluid contained in the stratum between the surfaces R S T and O P Q will be urged, by the gravitation at the latter surface, perpendicularly towards it. But the matter of the same stratum, besides pressing upon the surface O P Q, likewise attracts all the particles included within that surface. And the pressure and the attraction we have mentioned, are all the forces which the stratum exerts upon the fluid body O P Q. Now the body O P Q is *in equilibrio* in two different states: namely, when it is a part of the whole body R S T; and when it revolves by itself, the exterior matter being taken away: it must therefore be *in equilibrio* with respect to the difference of the forces which act upon it in the two states of equilibrium; that is, it must be *in equilibrio* by the pressure and attraction of the exterior stratum. And because the gravitation is perpendicular to the outer surface R S T, and likewise to the interior surface O P Q, indefinitely near the outer one, this latter must be a level surface, and the pressure of the exterior stratum upon any given space assumed in it must be every where the same †. Wherefore the body O P Q will be *in equilibrio* by the pressure of the exterior stratum acting separately; and consequently it must likewise be *in equilibrio* by the attraction which the same stratum exerts upon it. The stratum between the surfaces R S T and

* Prop. I.

† Let the equation of the outer surface be

$$\varphi = C:$$

then the equation of another surface indefinitely near it, which is perpendicular to all the lines to which the first is perpendicular, will be

$$\varphi = C - \delta C,$$

δC being a small variation of C. Let k denote the small distance of the two surfaces at any point, and p the gravitation at either extremity of k; then it is easy to prove that

$$k \times p = \delta C.$$

Now $k \times p$, or the product of the gravitation multiplied by the quantity of matter, is the pressure; and the equation shows that the pressure upon a given space is the same over all the surface.

O P Q

O P Q must therefore be possessed of such a figure as to attract all particles in the inside with equal force in opposite directions, because otherwise the fluid contained within it could not be *in equilibrio* with respect to the attraction of the stratum.

Since the taking away of the uppermost stratum will leave the remaining fluid bounded by the surface O P Q *in equilibrio*, it is evident that we may apply to the stratum immediately below that surface the same reasoning that has been applied to the stratum below the surface R S T. Wherefore the surface N L M will be a level surface, and the stratum between the surfaces O P Q and N L M will attract all particles in the inside with equal force in opposite directions. The same conclusion is equally true of all the successive strata. And because what has been proved of the several strata, taken separately, is manifestly true of the aggregate of any number of them, we are to conclude, that all the level surfaces of a homogeneous fluid *in equilibrio* by the supposed forces are similar to the outer surface, and that any stratum contained between two level surfaces attracts particles in the inside with equal force in opposite directions.

These two propositions determine all the conditions necessary to the equilibrium of a homogeneous fluid, the particles of which are subjected to a centrifugal force, and to a mutual attraction following the law observed in nature. They do not indeed apply immediately to a body composed of fluids varying in their densities; but, as the defect of the received theory of fluids is clearly pointed out, it becomes easy to deduce from the same principles a satisfactory solution of every case.

A problem in Natural Philosophy is not brought within the domain of the Mathematics, until all the physical conditions be previously investigated. It then becomes the province of the geometer to trace the necessary consequences of these conditions, and to deduce from them the required solution. When this natural order of research is not followed, the result is always attended with something imperfect, something not entirely satisfactory to the mind. The conditions of equilibrium being now fully known, we are able to discover why Maclaurin succeeded in proving that a homogeneous elliptical spheroid is *in equilibrio* by the attraction of its particles and a centrifugal force. The figure supposed, contains one of the conditions of equilibrium, and is the only figure compatible with that condition. Newton proved that a shell of matter, comprised between two similar, and similarly situated elliptical surfaces, or between two concentric spherical surfaces, will attract all particles in the inside with equal force in opposite directions:

rections: and this is precisely the new condition which, as we have proved, is necessary to the equilibrium of a homogeneous fluid. It may be observed that, of the two conditions of equilibrium, the one which relates to the attraction of the level strata upon particles within them, which is omitted in the received theory of fluids, alone determines the figure of the homogeneous body: and when this is fulfilled, as it is in the figure supposed by Maclaurin, it only remains to compute the attraction, and to adjust the centrifugal force so that the resultant of both shall be perpendicular to the surface. What is said of the researches of Maclaurin is equally true of the discoveries of Clairaut, as far as they relate to the figure of the planets. In the first part of his work on the figure of the earth, that excellent geometer has proved that the equilibrium of a mass of fluid cannot be safely deduced from the perpendicularity of gravity to the surface, which is the principle of Huyghens; nor from the balancing of the central columns, which is that of Newton; nor from the hypothesis of Bouguer, which requires the concurrence of both the principles just mentioned: and although, in place of these imperfect theories, he has substituted one drawn from more unexceptionable principles, which is liable to no objection except when there is a mutual attraction between the particles; yet, as the figure of the planets depend upon the very case in which the theory is defective, the main difficulties of the question were left untouched. In the second part of the work, which contains the author's discoveries relating to the figure of the earth, his investigations are not deduced *à priori* from his theory of fluids, but from the supposition of a figure either exactly, or very nearly, an elliptical spheroid.

If a homogeneous fluid composed of particles endowed with attractive powers be at rest, it will be *in equilibrio* when it has the figure of a sphere. Conceive that the spherical mass begins to revolve upon an axis with a velocity which impresses upon the particles a centrifugal force very small in proportion to the attractive energy; the globe will now flatten in some degree at the poles, and become protuberant at the equator. What is the exact form in which the fluid will thus dispose itself? Newton assumed, but without assigning any reason, that it is an elliptical spheroid of revolution. The discovery of Maclaurin verified the supposition of Newton; and as we are now in possession of all the conditions of the equilibrium of a homogeneous fluid, it is easy to prove that the revolving mass cannot be *in equilibrio* unless it have the figure which Newton supposed.

Let us now consider the matter a little differently, and with reference

reference to what is peculiar to the initial figure of the fluid. The sphere at rest possesses all the properties of a homogeneous mass *in equilibrio*; that is, the gravitation is perpendicular to the surface, and every particle is equally attracted in opposite directions by a hollow shell comprised between any two concentric surfaces. When a change is induced upon the sphere by a centrifugal force, the laws of equilibrium can be exactly fulfilled only when the new figure is an elliptical spheroid. But when the derangement of the spherical surface is very minute, it is obvious that both the conditions of equilibrium may be considered as fulfilled, if we preserve the perpendicularity of gravity to the surface. For the attractive property of the level strata being exact at first, the small variation of their figure induced by the centrifugal force, cannot be supposed to effect suddenly any perceptible deviation from the figure of equilibrium. Legendre first proved that a homogeneous fluid nearly spherical, revolving about an axis, and having the gravitation perpendicular to the surface, must be very nearly an elliptical spheroid. In the particular circumstances supposed, the true figure is thus found, not by a rigorous method, but approximately, although one of the conditions of equilibrium is left out. This is indeed true apparently; but in reality, as we have shown, both the conditions of equilibrium are very nearly fulfilled, and the more nearly the less is the deviation from the spherical figure. We may also object to the process of Legendre, that it substitutes a mathematical property which is true only in particular circumstances, and to a limited extent, in place of the physical principles that are universally true in all cases whatever.

The transition of the fluid sphere to an oblate figure nearly an elliptical spheroid, by the effect of a small centrifugal force, is not confined to the hypothesis of a mutual attraction of the particles in the inverse proportion of the square of the distance. It is likewise true if we suppose that every particle is attracted to the common centre by a force which is either the same at all distances from the centre, or varies as any power of the distance. In one particular law, namely, when the central force varies directly as the distance, the oblate figures are exactly elliptical spheroids, as Herman first proved in his *Phoronomia*.

The discoveries of Legendre on the attraction of spheroids, were greatly extended by Laplace. In the third book of the *Mécanique Céleste*, this subject is treated with the utmost generality, and with all the resources and all the elegance which the improved state of analysis is able to furnish. But it must be observed that, in applying this mathematical theory to the figure

figure of the planets, the principles of equilibrium laid down by Clairaut are supposed to be sufficient; and the same objections we have already noticed in the investigation of Legendre are equally applicable to the researches of Laplace. The speculations of this great geometer mark the limit of our attainments in one branch of the philosophy of Newton: on this account they are deserving of a more ample discussion than we can at present bestow upon them, and they will probably engage our further attention at some future time.

April 3, 1825. JAMES IVORY.

XL. *Correction of an Inadvertence in a Geodetical Problem inserted in the Philosophical Magazine for July last.* By J. IVORY, *Esq. M.A. F.R.S.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

IN the geodetical problem inserted in the Philosophical Magazine for July last, I find there is an inadvertency with regard to the designations of the angles ψ and λ, which are not the true latitudes, but angles having a very simple relation to the latitudes. But this circumstance affects neither the reasoning nor the accuracy of the formulæ, which are strictly true when the proper values of the angles ψ and λ are understood.

Conceive a semimeridian upon a diameter of the equator, and likewise a semicircle upon the same diameter; and from a point in the diameter produced draw tangents to both curves: then, the angle λ is the inclination of the tangent of the circle to the polar axis; and the true latitude, for which I shall put l, is the inclination of the tangent of the ellipse to the same axis. The angle λ is by some authors termed the *reduced* or *corrected* latitude; and my solution is true and correct if the reader will be pleased to observe that ψ and λ stand for the reduced latitudes, and not the true latitudes.

The property of the ellipse furnishes this equation,

$$\tan\lambda = \frac{\tan l}{\sqrt{1+e^2}}:$$

whence we get
$$\sin\lambda = \frac{\sin l}{\sqrt{1+e^2\cos^2 l}}$$

$$\cos\lambda = \frac{\cos l\sqrt{1+e^2}}{\sqrt{1+e^2\cos^2 l}}$$

and, very nearly,
$$\lambda = l - \frac{e^2}{4}\sin 2l.$$

These

These values make it easy to substitute l for λ if it be thought proper so to do. In my formulæ for deriving s' from s, and ϕ from ϕ', it is evident that λ and l are to be reckoned equivalent.

If we allow that $\sqrt{1+e^2\ \cos^2(l+\delta l)}$ is equivalent to $\sqrt{1+e^2\cos^2 l}$, we shall get by substituting the proper values in the expressions of sin ψ and sin μ',

$$\sin(l+\delta l) = \sin l \cos s' + \cos l \cos\mu \sin s' \sqrt{1+e^2}$$

$$\sin\mu' = \frac{\cos l \sin\mu}{\cos(l+\delta l)}.$$

In like manner, we obtain

$$\sin\phi' = \frac{\sin\mu \sin s'}{\cos(l+\delta l)} \times \frac{\sqrt{1+e^2\cos(l+\delta l)}}{\sqrt{1+e^2}};$$

or, very nearly,

$$\sin\phi' = \frac{\sin\mu\sin s'}{\cos(l+\delta l)} - \frac{e^2}{2}\sin\mu\sin s'\tan(l+\delta l)\sin(l+\delta l).$$

The advantage of my solution consists in its giving the exact relations between all the quantities concerned, without much calculation, and by means of expressions as simply as the nature of the case will admit. Hence, in applying it to practice, it is easy to push the approximations to any required degree of exactness.

I remain, sir, yours, &c.

April 5, 1825. JAMES IVORY.

N. B. I observe that in the second formula, reckoning from the top of p. 37, cos ψ is printed for $\cos^2\psi$.

XLI. *Observations on Locomotive Action, with reference to its Differences in Skaiting and Walking. By* A CORRESPONDENT.

UPON what principle is it that the space gone over in skaiting is so much greater than that gone over in the same time and with the same exertion in walking? The answer to this question would furnish curious hints as to an advantageous application of force on rail-ways, &c.

At first sight it might seem easy to answer, that the propelling impetus communicated by the muscles of the leg behind does not, in walking, carry the body beyond the spot where the other leg is set down, but is checked and momentarily stopped there by friction; whereas in skaiting, this impetus not only carries up the centre of gravity to that spot, but, owing to the smoothness and slipperiness of the skait-irons and ice, enables the body to glide forwards over a considerable space besides:

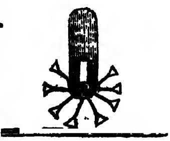
besides: each impetus thus causing a deal more ground to be passed over than in walking, where the progressive motion is at each step checked, and determined to the spot where the foot is set down. But such a solution is not consistent with a just idea of the nature of the action of walking. The velocity of progression of a man walking uniformly is not materially checked by any friction between his foot and the ground; nor is it momentarily stopped, as it were, and renewed at each step. It continues uniform, like the velocity of a boat uniformly rowed; *i. e.* is never much under or over a certain mean point. About the time of setting down the foot it is perhaps a trifle less than the mean rate; and just after the exertion of the muscles of the hinder leg it is a trifle more: so that, for example, if a man walk at the rate of four miles an hour, his velocity is sometimes a little more and sometimes a little less than that; and so keeps passing at every step from the somewhat more to the somewhat less, without ever being suddenly checked or stopped, or ever differing much from that mean velocity of four miles an hour, even for the smallest instant of time.

The fatigue in walking does not arise from the quantity of force *necessary* to propel the body (in *quantity* I include frequency of application), but from other causes connected with the machinery of animal bodies, which I will endeavour to explain; first observing that the truth of this assertion will be perceived indirectly, by considering that in walking downhill it is so far from being necessary to use force to propell the body, that, if the declivity be considerable, it is on the contrary necessary to check the tendency to accelerated velocity: so that if the necessary propelling force was the principal cause of fatigue on level ground, we ought, comparatively speaking, mile for mile, to be little exhausted in descending gentle pleasant declivities, where the ground is good and the footing firm; as, for example, on the mossy, short, elastic, yielding grass on the sides of some mountains. The additional shock in walking down-hill occasioned by the descent of the foot is, according to my experience, very trifling on such ground as I have mentioned. But if any one should maintain that, owing to this or other cause, walking down declivities cannot be compared with walking on level ground, let him imagine a man standing on level ground, and constantly (or by intervals, as he pleases) pressed forward by a proper force, properly applied against his back,—such an one as gently to force him to gather up his legs and walk, without using any propelling force of his own: Would this enable him to go so much further in a day, as to authorise us to conclude that the prin-

 cipal

cipal cause of fatigue in walking is the exertion of the neces-sary *propelling* force? I presume, not.

In order to explain the machinery of walking, let us imagine a body moving on a wheel of spokes (by which I mean one whose spokes are unconnected with each other, except at the centre of the wheel); and let us in the first in-



stance suppose this body standing at rest on two of its spokes, and then to be propelled forward by some external force. It is evident that while the forward spoke is becoming more and more erect, the (centre of gravity of the) body must rise; and that it must afterwards fall through the same degrees, till the succeeding or third spoke comes into contact with the ground: also, that if the ends or feet, as they may be called, of the spokes be properly shaped, the friction will be but a part of that in the case of a common wheel: also, that the force required to propel this body depends, *cæteris paribus*, on the infrequency of the spokes; because *that* determines the height to which the body will have to rise and fall, as well as the angle which the direction of its rising and falling will make with a horizontal line. This force is similar to that part of the force used in walking that is necessary simply to propell the body. Let us now further assimilate the action of our machine to that of walking, by supposing it propelled, not by an extraneous force, but by the elongation of the spokes, occasioned by the uncoiling of a spring or otherwise, and subsequent contraction to their original length.—Here we have a very near approximation to walking. Let us introduce one change more:—Let there be but two spokes; and let an internal machinery bring round the backward spoke after its elongation and subsequent contraction, and place it forward, so as to be ready to meet the ground as the next spoke. This third machine of ours will perfectly represent the action of walking, so far as concerns our present purpose. Now it is evident, as was observed before, that the first machine proceeds with less friction than a common wheel; *i. e.* if the ground be smooth, with very little, and that there is no sudden check or stop in the progressive movement. But whatever is true of the first, with respect to friction, &c. between the ground and foot of the spoke, is true of the second and third forms of the machine; therefore, &c. Transferring this to walking, we may see that neither is it true *there*, that the exertion required depends much on friction, or stoppage occasioned by setting down the foot. In fact, this exertion is composed of two parts; one of which is that necessary simply to propell the body, and

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which is the same as the force required in the above machines:—the other is the exertion necessary for gathering up and extending the limbs, and rolling them on the sockets of the joints, without progressive motion of the body; and is very much like that required for mimicking walking without stirring from the place; and still nearer, if not exactly, like that required when a person is pushed forward and made to walk. And it fatigues from the same causes that throwing about the arms fatigues;—the alternate contraction and relaxation of the muscles, even without effort, as it may be called, (*i. e.* without moving any weight except that of the limbs themselves,) exhausts their ready power; changes the *set* of the fibres; occasions wear and tear; and by various pressures accelerates the motion of the blood, &c. It is this division of the force I am now speaking of that causes all exertion of animals, whether man or other, in walking, trotting, &c. to be attended with so great a waste of strength. The power of a man to propell himself on a tolerable road would, with proper machinery, be greatly superior to what he has in walking.

In short, the locomotive powers of animals are at present very disadvantageously exerted, particularly in quick movements. But in all the machinery I have heard of, the inventors, by pertinaciously clinging to certain erroneous though natural ideas, do nothing but transfer the unnecessary exertion from one part of the body to another; or else they place the animal in such a position (as in the velocipedes, for instance, where they cling to the idea of the seat on a horse) as to work the muscles in the most inconvenient manner possible.

In skaiting, instead of having our progress determined by rolling round on our animal wheels, we, besides this rolling round, slide along the ground, with the limbs relatively at rest, and thereby to a given space have fewer movements *of the wheel;* though probably the propelling force exerted is nearly the same in both cases. Besides this, in most modes of skaiting the disturbances given to the relative situation of the limbs are not only much less frequent, mile for mile, than in walking, but are reduced within much smaller limits. There are, indeed, modes of skaiting in which these disturbances are reduced almost to nothing—where the legs have no alternate motion of passing each other; but these modes are not the most favourable, perhaps, to progress. If our third machine, instead of an impetus for each spoke, had one at every other, or every third, or every fourth, &c. sufficient to carry it round over the intermediate spokes, the indefinite extension given to the space gone over at each effort would be somewhat analogous to that in skaiting: and the same may be said if, in walk-

ing

ing, a man was pushed on so as to be forced to go two, three, four, or more steps involuntarily, as it may be called, at each push; or if he forced himself so to move by not exerting his propelling powers, except at every second, third, fourth, &c. time of taking up his leg from the ground.

The advantage of skaiting, then, is in avoiding much of that exertion which is necessary for moving the limbs among themselves, and which has nothing to do with the propelling force.

The practical corollaries I have to draw from these observations are very curious. I have for many years been anxious to realize them, but circumstances have forbidden me—they are still but theories. I think I could show a mechanician not only that the present mode of using animal power for locomotion is very uneconomical, but that there is a better mode feasible, and that *that* mode would be equally applicable to machines where steam, &c. is the moving power.

Z. A.

XLII. *On the Mode of obtaining Silicium, and on the Characters and Properties of that Substance. By M.* Berzelius*.

Decomposition of Fluate of Silica by Potassium.

WHEN we read the description of the experiments made by MM. Gay-Lussac and Thenard on the silicated fluoric acid and potassium, we cannot doubt that they decomposed the acid in the circumstances which they describe. The potassium burns in the gas and condenses it; a brown matter is formed, which, boiled in water and dried, burns in oxygen gas with the evolution of silicated fluoric gas, and leaves as a residuum a white earthy matter. In applying myself to these researches, I looked upon the reduction of the fluoric acid and of the silica as so certain, that I thought it only necessary to examine the composition of the product obtained, in order to exhibit the subject in a clear light. The experiment of MM. Gay-Lussac and Thenard gave me the same results as they obtained, with this difference; that the mass when burnt in oxygen gas was not white, and that it had preserved its primitive colour without any remarkable change. I expected to find some fluate of silica and potash, but the concentrated sulphuric acid did not disengage any trace of fluate of silica; and although carried to ebullition, it did not produce any change in the substance. Nor was the substance attacked by any acid but the fluoric, which separated the si-

* From the *Annales de Chimie et de Physique*, tom. xxvii. p. 337.

lica

lica, and left a deep brown matter, insoluble in acids, and which did not burn on exposure to the action of fire. Was this the radical of the fluoric acid, or that of silica, or a combination of both?

To procure this substance in greater quantity I proceeded in the following manner: I introduced into a glass retort of the capacity of about ten cubic inches a small vessel of porcelain on which was a piece of potassium of the size of a large nut. After having rapidly exhausted the retort I let in some silicated fluoric gas, from a reservoir over mercury, and I applied the heat of a spirit-lamp. The potassium at first became white, but afterwards it became browner and browner, and at last as black as a coal: it was not long in becoming inflamed, and it burnt with a large flame of a deep-red colour, not however intensely; whilst the mercury rose rapidly in the reservoir, on account of the absorption of the silicated fluoric gas. As soon as the combustion ended, a vacuum was made in the apparatus, in order to prevent the formation of the fluate of silica and potash, and it was then left to cool. The product was a hard, porous, agglutinated, and deep-brown mass, which did not change in the air, but which, like manganese, emitted the smell of hydrogen when it was pressed between the fingers or when breathed upon. In the retort, around the porcelain vessel, a light powder of a yellow-brown was remarked, which was preserved. The burnt mass when thrown into water immediately produced a copious evolution of hydrogen gas. The water dissolved much fluate of potash, and the brown mass was precipitated in the form of a chestnut-brown powder, slowly evolving gas. The alkaline solution was decanted and replaced by pure water. The evolution of gas visibly diminished; and the water having again been renewed after some time, it entirely ceased; so that the brown powder did not decompose the water, even at the temperature of ebullition. The solution obtained by boiling it being very acid, it was boiled with fresh portions of water, until no signs of acidity were manifested, and the solution afterwards passed through the filtre. The water was saturated with fluate of silica and potash, and the brown powder was washed as long as the water left the slightest residuum by evaporation. The powder, being dried, was of a chestnut-brown, and evidently contained heterogeneous parts of a brighter colour. The brown matter which was deposited on the glass during the combustion of the potassium was much more homogeneous. Water did not disengage hydrogen by its action, but quickly became acid. This powder was washed with the same care as the preceding.

Now, for the purpose of determining the change undergone by

by the brown matter during combustion, it was first perfectly dried and heated to a dull red in a current of hydrogen gas, and, after being weighed, exposed in a proper apparatus to a current of oxygen gas. As soon as the air of the vessel seemed to be displaced by oxygen gas, the brown matter was heated with a spirit-lamp: it immediately took fire, and burnt vividly for some time, having a pale blue flame on its surface. The remaining oxygen being passed into barytic water, rendered it very turbid, and gave a precipitate which entirely dissolved with effervescence in muriatic acid, and which, consequently, was carbonate of barytes. The burnt mass was greatly diminished in bulk, but was of much the same colour as before. Its weight was augmented scarcely half a hundredth part. There was not perceived either in the glass balloon where the combustion took place, or in the glass conducting tube, the least indication of the presence of fluoric acid; and this showed plainly that this acid was not produced by the combustion of the brown matter; and that the presence of it in the experiments of MM. Gay-Lussac and Thenard, as also in the first which I made, arose from the circumstance that the brown matter contained fluate of silica and potash, which was decomposed by the heat of the combustion, and gave out silicated fluoric acid. This result likewise deprives us of the hope of learning by these means the composition of fluoric acid; but it is not less interesting, since it seems to show that the pulverulent brown matter is really *silicium*. Its combustion in oxygen, with the production of carbonic acid, without much increase of weight, was now not so difficult to understand, since it is a property of the oxides which contain three atoms of oxygen, that their quadri-carburet burns without increase of weight. But whence comes this carbon? how is it combined with the silicium? I thought at first that it might be attributed to the presence of the oil of naphtha in which the potassium was preserved, and I repeated the experiment with potassium which had not been in this oil. The result was the same. I then began to think that this metal might contain carbon; for it had been prepared after the advantageous process lately given by Brunner, which consists in distilling at a high temperature a mixture of carbonate of potash and of carbon in a vessel of forged iron. The potassium, distilled in a glass vessel, accordingly left a coally residuum, which, by treatment with water, yielded some potash and much carbon. In repeating the experiment of the combustion of potassium in silicated fluoric gas [employing the metal thus purified], the powder was not so brown, and when burnt in oxygen acquired an increase of 40 per cent without producing carbonic

acid.

acid. Its colour, however, was much the same as before combustion. But this circumstance will no longer appear astonishing, if we suppose, either that the silicium may take in its combustion a lower degree of oxidation, or that, as happens with boron, the silica which is formed prevents the entire combustion of the silicium. The residuum of the combustion, treated by fluoric acid, gave silicated fluoric gas, and its colour became much darker. Thrown on a filter, well washed and dried, it was pure *silicium.*

Description of Silicium, and of its Chemical Relations with other Bodies.

Obtained in the manner just related, silicium is of a dark nut-brown colour, without the least metallic lustre. When rubbed with steel it does not give any bright streak, and resists the friction as an earthy body does. It is incombustible in atmospheric air and in oxygen; it does not undergo any change in the flame of the blowpipe, and appears to belong to the class of the most infusible bodies. These properties seem opposed to what I have already said of the combustion of silicium, which is easily accomplished when we employ that substance as obtained immediately after its reduction by potassium. This difference in its combustibility is extremely remarkable. It does not depend on an effect attributable to the fluoric acid; for, if before burning the silicium it is treated with fluoric acid, a portion of silica will be extracted from it, which had been separated from the fluoric acid by the potash produced by the oxidation of the potassium by the air, before the experiment could be made; and the acid dissolves besides, especially by means of heat, a portion of silicium, with evolution of hydrogen gas. The silicium which then remains after filtering and washing inflames and burns with vivacity in air and in oxygen. Nor can this combustibility result from a residuum of potassium; for after combustion neither fluoric acid nor fluate of silica and potash can be extracted from the product: it appears to me to be owing more probably to a portion of hydrogen combined with the silicium; for, if the silicium be burnt in oxygen, even after having been heated in hydrogen or in a vacuum, water will be formed, but in very small quantity relatively to the great capacity of saturation of the silica. The silicium which is obtained when the brown mass procured with potassium is thrown into water is, after that, a *hydruret of silicium* or a *siliciuret of hydrogen:* the reduced mass is a *siliciuret of potassium,* which the water decomposes; the potassium is converted into potash and dissolved; the greater part of the hydrogen is disengaged, and the smaller part takes the

 place

place of the potassium in combining with the silicium. If the hydruretted silicium is put into an open platinum crucible, and slowly heated until redness commences, and then after being covered carried to a white-red heat, the silicium loses its combustibility; and treated with fluoric acid, which now dissolves no more of it, becomes perfectly pure, without sustaining the great loss which takes place when we begin by burning it.—When the hydruretted silicium is very quickly brought to a red heat, it takes fire, because the hydrogen cannot burn at a lower temperature without inflaming the silicium at the same time. If the silicium does not burn completely in this case, that circumstance does not arise from the production of an inferior degree of oxidation, but from the access of oxygen being prevented by the silica formed. Besides the loss of hydrogen which the silicium sustains at an elevated temperature, it undergoes a further change; it loses the property of dissolving in fluoric acid, is contracted in bulk, and takes a darker colour. This circumstance has certainly as great an influence in diminishing its combustibility as the loss of its hydrogen. In the state of least condensation in which it is obtained, when just separated from the potassium by means of water, it may be compared, as to its combustibility, to the porous hydrogenated charcoal of lint, which takes fire by the sparks of a steel, but which loses this property after having been exposed to an elevated temperature. The incombustibility of silicium is besides so great, that the small quantity of it which remains on the filters may be found by burning them and treating the ashes with fluoric acid.

Silicium, even when dry, stains and strongly adheres to the glass vessels in which it is kept. When it is treated with fluoric acid, the liquid is covered with a little pellicle, which envelops, as fat oils do, each drop that is taken from it. This pellicle rises on the sides of the vessels so long as they are damp, and then appears, by refraction, of a clearer colour than the silicium which is under the liquid.

Silicium is a non-conductor of electricity. That which has become incombustible by its exposure to a strong heat does not undergo any change, if, whilst red-hot, chlorate of potash is thrown upon it. It is not attacked by saltpetre until the temperature is raised sufficiently to decompose the nitric acid, and the affinity of the alkali begins to act; but at a white heat it is acted upon with great energy by that salt.

With carbonate of potash, silicium burns very easily with a vivid inflammation; carbonic oxide is given out, and the reduced carbon imparts a black colour to the mass. The incandescence is so much the more intense, and the temperature

requires

requires to be so much less elevated in order to determine the action, as less carbonate of potash or of soda is taken. Thus, for example, taking a portion of carbonate equal in bulk to half the silicium, the inflammation takes place much below the temperature of ignition. With greater quantities of the carbonate, the mass becomes distended by the production of the carbonic oxide, takes fire, and burns with a blue flame. If a still greater quantity of carbonate be employed, no signs of combustion can be perceived; the mass does not grow black, and merely gives out carbonic oxide.

From this mode of action of silicium with carbonate of potash arises a very paradoxical phænomenon. If some incombustible silicium is heated with saltpetre to a moderate red, on a leaf of platinum or in a little crucible, no action will be observed; but if there is added a little dry carbonate of soda, so that it may touch the silicium, a detonation will take place, at the expense of the carbonate, in the midst of the saltpetre, and the mass will preserve for some time its black colour.

The cause from which the silicium burns at a low temperature more easily with the carbonate than with saltpetre, is without doubt owing to the affinity of the alkali for silica being necessary to determine the combustion of the silicium, and not being able to manifest itself with the saltpetre except when the temperature is sufficiently elevated for the decomposition of the nitric acid. If the burnt mass continues black some time longer, this arises from the new combination being compact, and protecting the carbon until it enters into fusion.

Silicium detonates with the hydrates of the fixed alkalies, producing a vivid incandescence at a temperature at which they melt, but much below that of ignition. Hydrogen gas is given out, which is seen to burn when the bulk of the mass is not too small. The same phænomena are observed with the hydrate of barytes. Incandescence also takes place with the hydrate of lime; but it is very feeble, and the silicium is oxidated but imperfectly. With the acid fluate of potash it detonates at the fusing temperature of the salt, that is to say, much below a red heat: it is not affected by fused borax.

Silicium heated to a perfect red in the vapour of sulphur inflames and burns, but with less intensity than in oxygen; and the combination even does not take place with the incombustible silicium. The sulphuration is usually as incomplete as the oxidation, and a scorified mass is obtained of a dark gray colour. It sometimes happens, however, particularly when a vacuum is made in the vessels before volatilizing the

 sulphur,

sulphur, that the silicium becomes completely sulphuretted, at least in a part of its mass. It then presents an earthy white body, which, thrown into water, instantly dissolves in it with the evolution of sulphuretted hydrogen. The silicium is converted into silica which dissolves in the water; and if this is in small quantity, a solution may be obtained so much concentrated, that it solidifies after a slight evaporation, and it leaves the silica, after the desiccation, in a transparent fissured mass. Silicium imperfectly sulphuretted also decomposes water rapidly, with the disengagement of hydrogen, and solution of the silica in the water; and the silicium which was not combined with the sulphur becomes separated. In the air the sulphuret of silicium diffuses a very strong smell of sulphuretted hydrogen, and loses in a little time all its sulphur; but in dry air it may be preserved a long time. At a red heat, it contracts and shrivels up, yielding sulphurous acid and silica. This change, however, takes place but slowly; for when kept at a red heat for some moments, it still has the property of decomposing water.

The siliciuret of potassium readily combines with sulphur at a red heat; but if the mass is dissolved in water, there remains much silicium, unless the mass be newly exposed to a white heat, because the silicium then combines with the sulphur at the expense of the potassium previously sulphuretted at a more elevated temperature. This combination is a true double sulphuret; its colour is of a dark brown, almost black. It presents a melted mass which dissolves in water. It is difficult to say whether it dissolves without changing its nature; but since the sulphuret of silicium is decomposed in water, and sulphuretted hydrogen has a great affinity for the sulphuret of potassium, it is very probable that in the solution there exists some silicate of potash with hydro-sulphuret of the same alkali. The combinations of the sulphuret of silicium with the metallic sulphurets, although they exist in the dry way, cannot, it appears, exist in water.

It is certainly a very remarkable fact to see the silica dissolve in so great quantity in water at the moment of its formation, and lose this property by evaporation, so as to become insoluble in acids. This great solubility, shown by the preceding experiments, explains the numerous crystallizations of silica in the cavities of druses, which sometimes could not contain a volume of liquid ever so little greater than the crystal itself. Nevertheless, I do not pretend that the silica had been in solution as in our example; that is to say, that it was the result of the decomposition of the sulphuret of silicium.

I did not succeed in combining phosphorus with silicium by

by bringing it into contact with the latter at a red heat. I did not try any other process.

If silicium is heated in a current of chlorine, it takes fire and continues to burn. If the gas contains atmospheric air, some silica remains in the form of a slight skeleton. Silicium burns equally well in chlorine, whether or not it has lost its combustibility in the air. The product condenses and presents a yellowish liquid when it contains an excess of chlorine, but which is without colour when it is freed from this excess. This liquid is very fluid; it evaporates almost instantaneously when exposed to the air, yielding white vapours, and leaving a residuum of silica. It has a very penetrating smell, which may be compared in some degree to that of cyanogen. Thrown into water, it floats on the top: it generally dissolves in it, or leaves a little silica. If the quantity of the water is small, a drop, for example, on as much chloride of silicium, this envelops it, and the silica remains in a frothy semi-transparent mass. This liquid is analogous to the combinations of the other electro-negative bodies with chlorine. It reddens litmus paper: it is the second example known of a liquid combination formed by silicium.

At common temperatures potassium has no action on chloride of silicium; but when heated in the vapour of the latter substance it takes fire and produces a compound of potassium and of silicium. The iodide of potassium does not combine with silicium.

Silicium is neither dissolved nor attacked by the sulphuric, nitric, or muriatic acids, and not even by aqua-regia. In its combustible state it is slowly dissolved by fluoric acid with the evolution of hydrogen; but in losing its combustibility it loses also the property of dissolving in this acid. It is, on the contrary, dissolved with rapidity, even in the cold, by a mixture of the nitric and fluoric acids, nitrous gas being given out. Combustible silicium is dissolved by digestion in a solution of caustic potash; but when rendered incombustible, it is no longer attacked by the alkalies in the humid way.

Silicium, when once isolated, combines with the metals with much difficulty. Its remarkable affinity for platinum is known by the experiments of M. Boussingault; but it may be heated as often and as long as we choose in a platinum crucible, without any combination taking place. But if we endeavour to reduce silicium by potassium in a platinum crucible, the silicium penetrates deeply into the platinum wherever this is touched by the potassium. Copper, silver, lead and tin, heated with silicium by the blowpipe, do not seem changed in their appearance, nor in their ductility; notwithstanding when

when they are treated with acids they leave a small quantity of silica. The copper, particularly, left a skeleton of the same form as its own. It is here to be remarked that silicium, which alone is not attacked by the acids, is oxidated by them when its combinations with the metals are dissolved. We have, however, a similar example already in rhodium, which, though not attacked *per se* by aqua-regia, is dissolved by it when alloyed with certain metals.

Titanium, which approaches so near to silicium in its properties, is also insoluble in acids in the metallic state (with the exception of a mixture of the fluoric and nitric acids), whilst it is oxidated and easily dissolved when alloyed with other metals.

Silicium combines with potassium at a high temperature, but without the evolution of a remarkable heat. It affords two combinations: one, with excess of potassium, of a dark-gray, and which dissolves completely in water; the other, with less potassium, is obtained by the reduction before stated, or by exposing the first to a very strong heat. It is besides probable that silicium may form with the metals combinations, the proportions of which correspond with those of their silicates.—I reserve these researches for another time.

Preparation of Silicium.

The combustion of potassium in silicated fluoric gas requires apparatus which we have not always at hand. The double salts, on the contrary, which fluoric acid forms with silica and potash, or soda, afford very advantageous means of preparing silicium. I did not observe any difference in the use of one or the other of these salts. That of soda has however the advantage of containing under the same weight and the same volume a greater quantity of the fluate of silica. This salt is used in the following manner:—It is reduced to a fine powder; and in case the desiccation has made its particles adhere, and also to expel the humidity that may remain, it is exposed to a temperature as high as it can bear without being decomposed, that is to say, much above 100°. It is then put by layers with the potassium into a tube of glass closed at one end, which is placed in such a manner as that all the mass may be heated at the same time. We may, if we choose, melt the potassium and mix it, by means of a rod of iron, with the salt, and then heat the mass over a spirit-lamp. Even before a red heat, the silicium is reduced with a slight hissing and a feeble appearance of heat. No gas is disengaged when the salt has been well dried. The mass is left to cool, and then treated as before described. It ought however to be dissolved

in

in a great quantity of water; so that the liquid, which becomes alkaline by the oxidation of the potassium, may be as diluted as possible, and have less tendency to oxidate and dissolve the silicium. It is for this reason that the mass ought not to be treated with hot water,—that the liquid has not lost its alkaline quality by the washing. It is afterwards boiled with water, and washed with hot water until no stain is left by the evaporation of a drop of liquid. For this purpose much time is generally requisite, and much water must be used. The silicium obtained by this process contains hydrogen; but in a smaller quantity, however, and perhaps in the same manner as the common charcoal of wood, which Davy considers as hydrogenated carbon. It contains also silica, which proceeds principally from the potassium before the reduction, being oxidated a little, and then separating a quantity of silica corresponding to the alkali formed: but the alkali which is formed after the reduction, when it dissolves the mass in the water, dissolves a portion of the double salt in excess, without separating the silica from it. This silica ought to be taken away by means of fluoric acid; but as the silicium would dissolve in the acid, we must begin by rendering it insoluble and incombustible: if it were made to burn in air, a portion of incombustible silicium would indeed be obtained after treatment by fluoric acid; but ordinarily two-thirds of it would be lost by the combustion. This loss is prevented by heating nearly to ignition the dried silicium containing hydrogen in an open crucible: this degree of heat is maintained some time, and then it is raised little by little to a perfect red heat. If the silicium become inflamed, the crucible should be covered directly, and the temperature lowered, which will instantly stop the combustion. When the calcination is finished, the silicium is incombustible in air, and is no longer attacked by fluoric acid, if it does not contain any foreign metal, iron or manganese for example; for in this case the alloy would be entirely dissolved with disengagement of hydrogen. After treatment by the acid, the silicium is washed and dried. It might be thought that this incombustibility is caused by an extremely thin pellicle of silica; but I dried the silicium in a vacuum, and then heated it to redness in the air, and I did not find any change of weight.

The silica may be reduced by heating it with potassium: but it happens either that the combination, rich in potassium, becomes entirely dissolved in the water; or else, when the heat is sufficient to expel the excess of potassium, that the silicate of potash formed melts into a vitreous mass, and envelops the silicium, which thence acquires a clearer colour. Part of the

the silicate may be separated by means of water, but the remainder can only be removed by fluoric acid. The quantity of silicium obtained is extremely small; and this manner of obtaining it merits attention only because it gives the same result as the double salt when treated with potassium; which proves that the evolution of hydrogen that takes place when the mass is treated by water is owing to the potassium, and not to the combustible radical of fluoric acid. It is by the process in question that Davy endeavoured to reduce silica; and he obtained with the mixed silicate of potash only a brown pulverulent matter, which dissolved in the water, giving it a greenish-gray colour. I observed the same colour in the solution; but it disappears when this becomes clear.

I passed silicated fluoric gas through a red-hot iron tube filled with turnings of the same metal, and it was not sensibly absorbed. The turnings when taken out were of a nut-brown colour, similar to that of silicium, and had the taste of fluate of iron, in the places where the heat had had the most intensity. This last having been dissolved by water, a perceptible pellicle of silicium was left on the surface of the iron, but so thin that it was not possible to separate it. We see by this experiment that iron at a sufficiently elevated temperature has an affinity strong enough to decompose the gas; but that the decomposition soon ceases, because the silicium which is deposited at the surface of the metal prevents its action. By fusing in a covered iron vessel a mixture of very fine iron filings with fluate of silica and potash, the salt was decomposed and converted into double proto-fluate of iron and potash: after having dissolved it in warm water, a combination of iron and of silicium was left. I hoped to be able to separate the iron by means of an acid; but the silicium became oxidated at the same time, although I employed the liquid silicated fluoric acid. In drying this alloy it oxidated more quickly in the air than it dried, and was changed into an ochre of a rusty yellow colour.

Composition of Silica.

Since silicium can be obtained in a direct manner, it was natural to endeavour to ascertain its oxidation in a direct manner. For this purpose I burnt 100 parts of pure silicium, dried in a vacuum, by heating it with carbonate of soda. The mass, treated by muriatic acid, evaporated to dryness, and strongly heated, was dissolved in water, and left some silica coloured gray by carbon, which, well washed and ignited, became snow-white, and weighed 203·75. The solution obtained and the waters of the washing were evaporated afresh, and the saline residuum

residuum heated to redness. Treating this with water, it still gave a little silica, which, after the addition of a drop of ammonia, took at the expiration of some hours a colour inclining to a yellowish-brown: when heated to redness it weighed 1·5; it had lost its dark colour, but it was not snow-white. With soda it gave on platinum foil a faint but evident trace of manganese. 100 parts of silicium had consequently absorbed 105·25 of oxygen, and produced 205·25 of silica. The experiment was repeated on a portion of silicium on which fluoric acid had been evaporated, in order to be more certain that all the silica had been removed. 100 parts of silicium, previously calcined to redness in the air, gave, by the process described above, 207 of silica; and by evaporation of the waters, 1 part; or in all, 208 of silica.

According to these two experiments, silica is composed of

Silicium	48·72	to	48·08
Oxygen	51·28	to	51·92

They both give a greater proportion of oxygen than that which I found, according to the capacity of saturation of silica for saline bases, and which is only 50·3.

But if we return to the analysis of the salts containing fluate of silica, we shall be able to calculate the saturating capacity of silicium according to the results which we have obtained from it.

Of all these double salts, that of barytes is the most suitable. The only uncertainty which could be met with in its analysis arises from its retaining a little humidity which is not disengaged until the moment of its decomposition. But the quantity of it may be determined by melting the salt with oxide of lead; for the acid is retained, and the water only disengaged. 100 parts of the salt lost in this manner 0·85 of moisture. 100 parts of the same salt, weighed at the same time, gave, according to the process described for the analysis of the fluate of silica and barytes, 82·933 of sulphate of barytes, representing 54·428 of barytes. But according to the analysis already known of the double fluate of silica and barytes, the base is combined with three times more fluoric acid than in the neutral fluate. It thence follows that the double salt is formed of

Barytes	54·428
Fluoric acid	22·836
Silica	21·886
Moisture	0·850
	100·000

The 54·428 parts of barytes are saturated by 7·612 of fluoric acid; whence it follows that 15·224 of this acid were

 combined

combined with 21·886 of silica, or that 100 of acid took 143·76 of silica. The fluate of silica is consequently composed of

Fluoric acid	41·024	100·00
Silica	58·976	143·76

But 100 parts of fluoric acid correspond to 74·7194 of oxygen in each base that it saturates; consequently this quantity of oxygen ought to be contained in the 143·76 of silex which the acid saturates, and the composition of silica should be

Silicium	48·025	100·00
Oxygen	51·975	108·22

This proportion comes very near to that obtained by synthesis, but it is difficult to say on which side the greatest errors of observation are to be found. According to what precedes, the weight of the atom of silicium, supposing that the silica contains three atoms of oxygen, would be 277·2, and, according to the best of the two synthetic experiments, 277·8. The other would carry it to 285; but this number by all appearance is too great. It exceeds by 1⅔ per cent that which had been adopted at first; which is so well suited for the new and most exact analyses of minerals, that if they were calculated according to the determination which we have just given, they would render an excess of silica necessary. But I must remark on this head, that a mineral is rarely found of which silica does not form an essential component, which does not yet contain from ½ to 2 per cent of it, and even more, either in the state of quartz or in that of another siliceous mineral. This circumstance must take place so much the more with the minerals which have silica for one of their elements; and consequently all that quantity of it which is found above what the calculation gives should be attributed to a siliceous mixture existing in the mineral.

As to the number of atoms of oxygen contained in silica; the new facts do not furnish us with the means of determining this point. The circumstance that the carburetted silicium produces when burned an equal weight of silica, leads us to consider it as a quadricarburet, which by burning would give an oxide at three atoms of oxygen; but as I have not been able to isolate the carburet of silicium, nor to burn it completely, this result, although obtained in several experiments, and with silicium produced by different operations, has not the certitude it ought to have, to be admitted as a proof. It ought notwithstanding to be considered as affording a strong presumption that silica contains three atoms of oxygen, so far as our knowledge respecting the crystalline form of bodies furnishes us with the means of deciding on the number of atoms of which oxides are formed. For the expression of the composition

position of the silicates by formulæ, it would certainly be more simple to consider silica as formed of an atom of silicium and an atom of oxygen; but it then would be difficult to conceive the existence of the silicates which contain six times the oxygen of the base, as in the apophyllite,—in which an atom of potash would then be combined with 12 atoms of silica.

To conclude: It still remains to be decided to what class of simple bodies silicium belongs. Since it has no lustre, nor the property of conducting electricity in the state in which it has hitherto been obtained, it is evident that it cannot be classed among the metals, and that its properties bring it near to boron and carbon. Some systematical philosophers will in consequence, doubtless, give it the name of *silicon*, to indicate by its termination the class of combustibles to which it should be referred. But I look upon this denomination as useless; for there is not any true limit between the metals and the metalloids. Carbon possesses the metallic lustre, and conducts electricity, but it is not considered as a metal; and if silicium could be melted, it then perhaps would have these properties, which it does not possess in a pulverulent state. Uranium, under this last form, can be distinguished but with difficulty by its appearance from silicium; and when crystallized, on the contrary, it has the metallic lustre and is transparent on the thinnest edges. Columbium and titanium also approach silicium by their chemical properties; and would it not be well to separate them from the metals, in order to unite them to the metalloids,—that is to say, to the non-metallic combustible bodies? I only wish to show, by these remarks, that there is no natural limit between these bodies; and that when their electric relation only is considered as exact, it is quite indifferent whether we place a combustible body among the metals or not.—(*Annalen der Physik und Chemie.*)

XLIII. *A Letter from the Rev.* W. Kirby *in explanation of his Remarks upon the Notice, given in the Philosophical Magazine, of Mr.* W. S. MacLeay's *Paper on the Tarsi of certain Insects.*

To the Editor of the Philosophical Magazine and Journal.

Dear sir, Webb's Hôtel, Piccadilly, April 14, 1825.

IT gives me great pain and concern to learn that some gentlemen have regarded my remarks upon your statement of the object of Mr. W. S. MacLeay's paper "On the Structure of the Tarsus in the Tetramerous and Trimerous Celeoptera of the French Entomologists," in a light very different from what I intended, and as having the air of an attack upon him. I hasten

hasten therefore to remove any misconception of this kind, both for his sake and my own; for nothing was further from my intention than to *attack* a friend whom I have so long and so highly esteemed, both for his personal qualities, his natural talents, and the extensive range of his learning and knowledge.

That I could have no such intention will be evident when I state that in the first instance I sent him the substance of the remarks which you have inserted in your Magazine, with the exception of that relating to De Geer, requesting him to put them into any form that might be agreeable to him, and insert them himself; but as it appeared to him that this would be committing himself with the Linnæan Society, he declined it.

With regard to De Geer, I regret exceedingly that I inadvertently and *too hastily* assumed that Mr. MacLeay was not aware of his having mentioned and figured the accessory joint at the base of the claw-joint of *Coccinella*; an error into which I was led by the mode in which the subject of Mr. MacLeay's paper was stated, and by the circumstance of having myself, subsequently to my first letter, observed to him that De Geer (of which, as I said, I was not then any more than himself aware) had noticed the joint in question.

I hope this will be deemed in some degree an extenuation of my error in building upon a foundation apparently so slight.

I must here also observe, that De Geer invariably looked upon *Coccinella* as being *trimerous*, as appears in almost every passage in which he has mentioned it, and in the explanation of this very plate; so that he regarded this not as a *primary* but as a *secondary* joint, or the joint of a joint, as I am disposed to do myself; and therefore in the Introduction to Entomology, and upon other occasions, I speak of the *Chysomelidæ*, &c. as *tetramerous*, and the *Coccinellidæ* as *trimerous*.

I also regret, when I said that the third volume of the work just alluded to had been "some time printed," that I did not add, "but not yet published." But since this last fact appeared to me to be universally known, it did not occur to me that this was necessary. It has, however, been observed to me, that some readers of your Magazine may possibly not have been aware of this circumstance. To do away any impression that may have thus been produced, I beg leave to state that Mr. MacLeay has never seen that part of my work in which the observations on the tarsi of tetramerous and trimerous insects copied in your Magazine are contained, and that I believe him incapable of knowingly appropriating to himself the honour that belongs to another.

I am, dear sir, yours, &c.

Wm. Kirby.

XLIV. *On the Action of finely-divided Platinum on Gaseous Mixtures, and its Application to their Analysis.* By Wm. Henry, *M.D. F.R.S.**

SEVERAL years have elapsed since the President of the Royal Society, in the further prosecution of those researches on flame which had already led him to the most important practical results, discovered some new and curious phænomena in the combustion of mixed gases, by means of fine wires of platinum introduced into them at a temperature below ignition. A wire of this sort being heated much below the point of visible redness, and immersed in a mixture of coal gas and oxygen gas in due proportions, immediately became white-hot, and continued to glow until all that was inflammable in the mixture was consumed. The wire, repeatedly taken out of the mixture and suffered to cool below the point of redness, instantly recovered its temperature on being again plunged into the mixed gases. The same phænomena were produced in mixtures of oxygen with olefiant gas, with carbonic oxide, with cyanogen, and with hydrogen; and in the last case there was an evident production of water. When the wire was very fine, and the gases had been mixed in explosive proportions, the heat of the wire became sufficiently intense to cause them to detonate. In mixtures which were non-explosive from the redundancy of one or other gas, the combination of their bases went on silently, and the same chemical compounds were formed as by their rapid combustion.

Facts analogous to these were announced in the autumn of last year by Professor Dœbereiner, of Jena, with this additional and striking circumstance,—that when platinum in a spongy form is introduced into an explosive mixture of oxygen and hydrogen, the metal, even though its temperature had not been previously raised, immediately glows, and causes the union of the two gases to take place, sometimes silently, at others with detonation. It is remarkable, however, that platinum in this form, though so active on mixtures of oxygen and hydrogen, produces no effect, at common temperatures, on mixtures of oxygen with those compound gases, which were found by Sir Humphry Davy to be so readily acted upon by the heated wire. Carbonic oxide appears indeed, from the statement of MM. Dulong and Thenard, to be capable of uniting with oxygen at the temperature of the atmosphere, by means of the sponge; but though this is in strictness true, yet the combination, in all the experiments I have made, has been extremely slow, and the due diminution of volume has not

* From the Phil. Trans. for 1824, Part II.

been completed till several days have elapsed. On mixtures of olefiant gas, of carburetted hydrogen, or of cyanogen, with oxygen, the sponge does not by any duration of contact exert the smallest action at common temperatures.

It was this inefficiency of the platinum sponge on the compounds of charcoal and hydrogen in mixture with oxygen, while it acts so remarkably on common hydrogen, and also, though slowly, on carbonic oxide, that suggested to me the possibility of solving by its means some interesting problems in gaseous analysis. I hoped more especially to be able to separate from each other the gases constituting certain mixtures, to the compositions of which approximations only had been hitherto made, by comparing the phænomena and results of their combustion with those which ought to ensue, supposing such mixtures to consist of certain hypothetical proportions of known gases. It might, for instance, be expected that from a mixture of hydrogen and carburetted hydrogen with oxygen, the platinum sponge would cause the removal of the hydrogen, leaving the carburetted hydrogen unaltered. To ascertain this and a variety of similar facts, I made artificial mixtures of the combustible gases in known volumes; and submitted them, mixed with oxygen, sometimes to contact with the sponge, and sometimes with the balls made of clay and platinum, described by Professor Dœbereiner.

Section I.

On the Action of finely-divided Platinum on Gaseous Mixtures at common Temperatures.

I. *Mixtures of Hydrogen and Olefiant Gases with Oxygen.*

When to equal volumes of olefiant gas, and an explosive mixture (which is to be understood, whenever it is so named, as consisting of two volumes of hydrogen and one of oxygen gases), one of the platinum balls, recently heated by the blowpipe, and allowed to cool during eight or ten seconds, is introduced through mercury, a rapid diminution of volume takes place; the whole of the hydrogen and oxygen gases is condensed; but the olefiant gas is either not at all or very little acted upon. In a few experiments when the tube was narrow, and the quantity of mixed gases small, the olefiant gas escaped combustion entirely; but, in general, an eighth or tenth of it was converted into water and carbonic acid. It is difficult, however, to state the precise proportion of any gas which, when added to an explosive mixture, renders the latter insensible to the action of the balls or sponge; for much depends on

on their temperature when introduced into the gaseous mixture, the diameter of the containing vessel, and other circumstances, which, in comparing different gases, should be so regulated as to be equal in every case.

When the proportions of the gases are changed, so that the explosive mixture exceeds in volume the olefiant gas, there is a more decided action upon the latter, manifested by an increased production of carbonic acid. Thus, for example, the explosive mixture being to the olefiant as $2\frac{1}{2}$ to 1, about one-fourth of the olefiant gas was consumed; and by increasing the proportion of the explosive mixture the olefiant gas was still more acted upon. On using oxygen sufficient to saturate both the hydrogen and the olefiant gases, the ball acted much more rapidly: in several instances it became red-hot; all the hydrogen was consumed; and the whole of the olefiant gas was changed into water and carbonic acid. In this case the use of the sponge is inadmissible, as it kindles the gases, and occasions their detonation.

II. *Mixtures of Hydrogen and Carburetted Hydrogen Gases with Oxygen.*

When carburetted hydrogen, procured from stagnant water, was added to an explosive mixture, in various proportions between equal volumes, and ten of the former to one of the latter, the action of the hydrogen and oxygen on each other took place as usual, on admitting one of the balls. When, reversing the proportion, the explosive mixture was made to exceed the carburetted hydrogen, but not more than four or five times, the latter gas was entirely unchanged. With a larger proportion of the explosive mixture carbonic acid was always found to have been produced; but still the carburetted hydrogen was very imperfectly consumed; and fully three-fourths of it were generally found to have escaped unburned.

When to a mixture of hydrogen and carburetted hydrogen, oxygen enough was added to saturate both gases, the effect of the sponge was found to vary with the proportion of the simple hydrogen. In several cases, where the hydrogen did not exceed the carburetted hydrogen more than four times, the latter gas remained unchanged; when in larger proportion, there was a decided action upon the carburetted hydrogen. But it was much more easy to regulate the action of the balls upon such a mixture so as to act upon the hydrogen and oxygen only, than in the case of olefiant gas, which, under similar circumstances, is always more largely converted into water and carbonic acid.

III. *Mixtures*

III. *Mixtures of Hydrogen and Carbonic Oxide with Oxygen.*

The addition of one volume of carbonic oxide to two volumes of an explosive mixture produces a distinct effect in suspending the action of the platinum balls, and even of the spongy metal itself. The action of the gases upon each other still, however, goes on slowly, even when the carbonic oxide exceeds the explosive mixture in volume; and after the lapse of a few days the oxygen is found to have disappeared, and to have partly formed water, and partly carbonic acid. I made numerous experiments to ascertain whether the oxygen, under these circumstances of slow combustion, is divided between the carbonic oxide and the hydrogen in proportions corresponding to the volumes of those two gases. The combustible gases being in equal volumes, and the oxygen sufficient to saturate only one of them, it was found that the oxygen which had united with the carbonic oxide was to that which had combined with the hydrogen as about 5 to 1 in volume. Increasing the carbonic oxide, a still larger proportion of oxygen was expended in forming carbonic acid. On the contrary, when the hydrogen was increased, a greater proportional quantity of oxygen went to the formation of water. But it was remarkable, that when the hydrogen was made to exceed the carbonic oxide four or five times, less oxygen in the whole was consumed than before; the activity of the carbonic oxide appearing to have been diminished, without a corresponding increase in that of the hydrogen.

In cases where the proportion of the carbonic oxide to the explosive mixture was intentionally so limited that the platinum ball was capable of immediately acting upon the latter, the carbonic oxide was always in part changed into carbonic acid, the more abundantly as its volume was exceeded by that of the explosive mixture. Increasing the oxygen, so that it was adequate to saturate both gases, and causing the hydrogen to exceed the carbonic acid in volume, a speedy action was always exerted by the ball, and the whole of the combustible gases was silently converted into water and carbonic acid. The introduction of the platinum sponge into such a mixture was almost always found to produce detonation.

IV. *Mixtures of Hydrogen and Cyanogen with Oxygen.*

When one of the platinum balls, after being recently heated, is introduced into cyanogen and explosive mixture in equal volumes, no apparent action takes place. With half a volume of cyanogen there is a slight diminution; and as we reduce the proportion

proportion of that gas, the action of the elements of the explosive mixture on each other becomes more and more distinct. There is not, however, as with carbonic oxide, any production of carbonic acid; but in the course of a few minutes the inside of the tube becomes coated with a brownish substance, soluble in water, and communicating to it the same colour; having a smell resembling that of a burnt animal substance, and yielding ammonia on the addition of a drop or two of liquid potash. It was produced in too small a quantity to enable me to submit it to a more minute examination; but its characters appeared to resemble those of a product obtained by M. Gay-Lussac by mixing cyanogen with ammoniacal gas.

If oxygen be added to a mixture of hydrogen and cyanogen, in quantity sufficient to saturate both the gases, it is still necessary, in order that an immediate effect should be produced by the sponge, that the hydrogen should exceed the cyanogen in volume. A decided action then takes place: an immediate absorption ensues; fumes of nitrous acid vapour appear, which act on the surface of the mercury; and after removing the nitrous acid by a drop or two of water, and transferring the gas into a dry tube, carbonic acid is found to have been produced, equivalent in volume to double that of the cyanogen.

V. *Effect of adding various other Gases to an Explosive Mixture of Hydrogen and Oxygen.*

It had been already ascertained by Professor Dœbereiner, that one volume of oxygen diluted with 99 volumes of nitrogen is still sensible, when mixed with a due proportion of hydrogen, to the action of the sponge. Carbonic acid also, even (I find) when it exceeds the explosive mixture ten times, retards only in a slight degree the energy of the sponge. Oxygen, hydrogen, and nitrous oxide gases, when employed to dilute an explosive mixture, are equally inefficient in preventing the mutual action of its ingredients. Ammonia may be added in ten times the volume of the explosive mixture, and muriatic acid gas in six times its volume, with no other effect than that of rendering the action of the sponge less speedy.

VI. *Mixtures of Carbonic Oxide and Carburetted Hydrogen with Oxygen.*

When mixtures of these gases are exposed to the sponge, the carburetted hydrogen seems to stand entirely neutral. The carbonic oxide is converted into carbonic acid, in the same gradual manner as if it had been mixed with oxygen only; and the carburetted hydrogen remains unaltered.

 VII. *Mix-*

VII. *Mixtures of Hydrogen, Carburetted Hydrogen, and Carbonic Oxide with Oxygen.*

In mixtures of these gases, it is of little consequence whether the oxygen be sufficient for the hydrogen and carbonic oxide only, or be adequate to the saturation of all three. The circumstance which has the greatest influence on the results of exposing such mixtures to the sponge, is the proportion which the simple hydrogen bears to the other gases, and especially to the carbonic oxide; for in order that there may be any immediate action, the former should exceed the latter in volume. In that case the hydrogen is converted into water, and the carbonic oxide into carbonic acid; but the carburetted hydrogen, unless the excess of hydrogen be very considerable, remains unaltered. If the proportion of hydrogen be so small that no immediate action is excited by the sponge, the ingredients of the mixture nevertheless act slowly upon each other; and after a few days the whole of the hydrogen and carbonic oxide are found to have united with oxygen, and the carburetted hydrogen to remain of its original volume.

VIII. *Mixtures of Hydrogen, Carbonic Oxide, and Olefiant Gases with Oxygen.*

When the oxygen, in a mixture of these gases, is sufficient to saturate the two first only, and the proportion of hydrogen is so adjusted that the action of the sponge is not very energetic, the hydrogen and carbonic oxide only are acted upon; but if the diminution of volume, which the sponge produces, be rapid and considerable, part of the olefiant gas is converted into water and carbonic acid. This effect on olefiant gas takes place still more readily if the oxygen present be adequate to the saturation of all three combustible gases.

It is remarkable, that if to a mixture of hydrogen, carbonic oxide, and oxygen, in such proportions that the sponge would act rapidly in producing combination, olefiant gas be added, the action of the gases on each other is suspended. Thus 20 measures of carbonic oxide, 31 of hydrogen, and 20 of oxygen, were instantly acted upon by the sponge; but the addition of 20 measures of olefiant gas to a similar mixture entirely suspended its efficiency. By standing fourteen days, rather more than half the carbonic oxide was acidified, and about one-twelfth of the hydrogen was changed into water, but the olefiant gas remained unaltered.

IX. *Mixtures of Hydrogen, Carbonic Oxide, Carburetted Hydrogen, and Olefiant Gases with Oxygen.*

In mixtures of these four gases with oxygen, it was found by

by varying the proportion of hydrogen, that hydrogen and carbonic oxide are most easily acted upon; then olefiant gas; and carburetted hydrogen with the greatest difficulty. When the action of the sponge was moderate, only the hydrogen and carbonic oxide were consumed, or at most the olefiant gas was but partially acted upon. Adding more hydrogen, so as to occasion a more rapid diminution, the olefiant gas also was burned; but the carburetted hydrogen always escaped combustion, unless the hydrogen were in such proportion that the ball or sponge became red-hot.

From the facts which have been stated, it appears that when the compound combustible gases, mixed with each other, with hydrogen, and with oxygen, are exposed to the platinum balls or sponge, the several gases are not acted upon with equal facility; but that carbonic oxide is most disposed to unite with oxygen; then olefiant gas; and lastly, carburetted hydrogen. By due regulation of the proportion of hydrogen, it is possible to change the whole of the carbonic oxide into carbonic acid without acting on the olefiant gas or carburetted hydrogen. With respect indeed to olefiant gas, this exclusion is attended with some difficulty, and it is generally more or less converted into carbonic acid and water. But it is easy, when olefiant gas is absent, so to regulate the proportion of hydrogen, that the carbonic oxide may be entirely acidified, and the whole of the carburetted hydrogen be left unaltered. This will generally be found to have been accomplished, when the platinum ball has occasioned a diminution of the mixture, at about the same rate as atmospheric air is diminished by nitrous gas when the former is admitted to the latter in a narrow tube.

Section II.

On the Effect of finely-divided Platinum on Gaseous Mixtures at increased Temperatures.

The effect of varying the proportion of free hydrogen to the compound combustible gases, on the degree of action which is excited by the platinum sponge, will perhaps admit of being explained, by examining the facts that have been stated, in connexion with the degrees of combustibility of the compound gases under ordinary circumstances. The precise degree of temperature at which any one of them burns is not known, on account of the imperfection of our present methods of measuring high degrees of heat. It has been ascertained, however, by Sir Humphry Davy, that at a heat between that of boiling mercury and that which renders glass luminous in the dark, hydrogen and oxygen gases unite silently, and with-

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out any light being evolved; that carbonic oxide is as inflammable as hydrogen; that olefiant gas is fired by iron and charcoal heated to redness; but that carburetted hydrogen, to be inflamed, requires that the wire should be white-hot. Now this is precisely the order in which the three compound gases require hydrogen to be added to them, in order to be rendered susceptible of being acted upon by the platinum sponge; carbonic oxide being acted upon with the smallest proportion of hydrogen, olefiant gas requiring more hydrogen, and carburetted hydrogen a still larger proportion. It is extremely probable then, that the temperature, produced by the union of the hydrogen and oxygen forming part of any mixture, is the circumstance which determines the combustible gases to unite, or not, with oxygen, by means of the sponge. It was desirable, however, to ascertain the exact temperature at which each of those three gases unites with oxygen with the intervention of the spongy platinum. For this purpose the gases, mixed with oxygen enough to saturate them, were severally exposed in small retorts containing a platinum sponge, and immersed in a mercurial bath, to a temperature which was gradually raised till the gases began to act on each other. In this way the following facts were determined.

1st. Carbonic oxide began to be converted into carbonic acid at a temperature between 300° and 310° Fahrenheit. By raising the temperature to 340°, and keeping it at that point for 10 or 15 minutes, the whole of the gas was acidified, the condensation of volume in the mixture being equivalent to the oxygen which had disappeared.

2dly. Olefiant gas, mixed with sufficient oxygen, and in contact with the sponge, showed a commencement of decomposition at 480° Fahrenheit, and was slowly but entirely changed into carbonic acid by a temperature not exceeding 520° Fahrenheit. MM. Dulong and Thenard state the same change to take place at 300° Cent. = 572° Fahrenheit; but having repeated the experiment several times, I find no reason to deviate from the temperature which I have assigned.

3dly. Carburetted hydrogen, exposed under the same circumstances, was not in the least acted upon by a temperature of 555° Fahrenheit, the highest to which, by an Argand's lamp, I was able to raise the mercurial bath. This, however, must have been near the temperature required for combination; for on removing the retort from the mercurial bath, and applying a spirit lamp, at such a distance as not to make the retort red-hot, a diminution of volume commenced, and continued till all the carburetted hydrogen was silently converted into water and carbonic acid.

4thly.

4thly. Cyanogen, similarly treated, was not changed at a temperature of 555° Fahrenheit; and on applying the flame of a spirit lamp to the tube, it produced no action till the tube began to soften.

5thly. Muriatic acid gas, mixed with half its volume of oxygen, began to be acted upon at 250° Fahrenheit. Water was evidently formed; and the disengaged chlorine, acting upon the mercurial vapour in the tube, formed calomel, which was condensed, and coated its inner surface.

6thly. Ammoniacal gas, mixed with an equal volume of oxygen, showed a commencement of decomposition at 380° Fahrenheit. Water was also in this case distinctly generated; and at the close of the experiment nothing remained in the tube but nitrogen and the redundant oxygen.

I proceeded, in the next place, to examine the agency of finely-divided platinum at high temperatures, on those mixtures of gases, which are either not decomposed, or are slowly decomposed, at the temperature of the atmosphere.

When carbonic oxide and hydrogen gases, in equal volumes, mixed with oxygen sufficient to saturate only one of them, were placed in contact with the sponge, and gradually heated in a mercurial bath, the mixture ceased to expand between 300° and 310° Fahrenheit, and soon began to diminish in volume. On raising the temperature to 340°, and keeping it some time at that point, no further diminution was at length perceptible. From the quantity of carbonic acid remaining at the close of the experiment, it appeared that four-fifths of the oxygen had united with the carbonic oxide, and only one-fifth with the hydrogen. When four volumes of hydrogen, two of carbonic oxide, and one of oxygen, were similarly treated, the hydrogen, notwithstanding its greater proportional volume, was still found to have taken only one-fifth of the oxygen, while four-fifths had combined with the carbonic oxide. These facts show that at temperatures between 300° and 340° Fahrenheit, the affinity of carbonic oxide for oxygen is decidedly superior to that of hydrogen; as, from the experiments before described, appears to be the case, also, at common temperatures.

But a similar distribution of oxygen, between carbonic oxide and hydrogen, does not take place, when those three gases are fired together by the electric spark. This will appear from the following table, in which the three first columns show the quantities of gases that were fired, and the two last, the quantities of oxygen that were found to have united with the carbonic oxide and with the hydrogen.

	Before Firing.			*After Firing.*	
	Measure of Carb. Oxide.	Measure of Hydrogen.	Measure of Oxygen.	Oxygen to Carb. Oxide.	Oxygen to Hydrogen.
Exp. 1.	40	40	20	6	14
2.	40	20	20	12	8
3.	20	40	20	5	15

When equal volumes of carbonic oxide and hydrogen gases, mixed with oxygen sufficient to saturate only one of them, were exposed in a glass tube to the flame of a spirit lamp, without the presence of the sponge, till the tube began to soften, the combination of the gases was effected without explosion, and was merely indicated by a diminution of volume and an oscillatory motion of the mercury in the tube. At the close of the experiment, out of twenty volumes of oxygen, eight were found to have united with the carbonic oxide, and twelve with the hydrogen—proportions which do not materially differ from the results of the first experiment in the foregoing table. At high temperatures, then, the attraction of hydrogen for oxygen appears to exceed that of carbonic oxide for oxygen: at lower temperatures, especially when the gases are in contact with the platinum sponge, the reverse takes place, and the affinity of carbonic oxide for oxygen prevails.

Extending the comparison to the attraction of olefiant and hydrogen gases for oxygen at a red heat, I found that when six volumes of olefiant, six of hydrogen, and three of oxygen were heated by a spirit lamp till the tube softened, a silent combination took place as before; all the oxygen was consumed, but only half a volume had been expended in forming carbonic acid, which indicates the decomposition of only one quarter of a volume of olefiant gas. On attempting a similar comparison between carbonic oxide and olefiant gas, by heating them with oxygen in the same proportions, the mixture exploded as soon as the glass became red-hot, and burst the tube.

The property inherent in certain gases, of retarding the action of the platinum sponge, when they are added to an explosive mixture of oxygen and hydrogen, is most remarkable in those which possess the strongest attraction for oxygen; and it is probably to the degree of this attraction, rather than to any agency arising out of their relations to caloric, that we are to ascribe the various powers which the gases manifest in this respect. This will appear from the following table, the first column of which shows the number of volumes of each . to render one volume of an explosive mixture of hydrogen and oxygen uninflammable by the discharge of a Leyden jar; while the second column shows the number of volumes of each gas necessary in some cases to render one volume

volume of an explosive mixture insensible to the action of the sponge, and in other cases indicates the number which may be added without preventing immediate combination. In the first column, the numbers marked with an asterisk were determined by Sir Humphry Davy; the remaining numbers in that column, and the whole of the second, are derived from my own experiments.

1 vol. of Explosive Mixture was rendered incapable of being inflamed by electricity when mixed with		Effect of adding the same gases to 1 vol. of Explosive Mixture on the action of the sponge.
About 8 vol. of	Hydrogen	not prevented by many vol.
6	Nitrogen	ditto
9	Oxygen	not prevented by 10 vol.
11	NitrousOxide	ditto
1·5	Cyanogen	prevented by 1 vol.
1	Carb^{zd}. Hyd.	not prevented by 10 vol.
4	Carb^{c}. Oxide	prevented by $\frac{1}{2}$ vol.
0·5	Olefiant Gas	prevented by 1·5 vol.
2	MuriaticAcid	not prevented by 6 vol.
2	Ammonia	not prevented by 10 vol.
3	CarbonicAcid	ditto

From the foregoing table it appears that carbonic oxide produces the greatest effect, in the smallest proportion, to an explosive mixture of oxygen and hydrogen, in preventing the action of those gases on each other, when exposed to the sponge at temperatures below the boiling point of mercury. In general those gases which either do not unite with oxygen, or unite with it only at high temperatures, have little effect in restraining the efficiency of the sponge. There is an apparent exception, however, in cyanogen, which it would require more research than I have yet had time to devote to an object merely collateral, to reconcile it, if it be capable of being reconciled, with the general principle.

From the fact that carbonic oxide, olefiant gas, and carburetted hydrogen, when brought to unite with oxygen by means of the platinum sponge, assisted by heat, undergo this change at different temperatures, it seemed an obvious conclusion, that by exposing a mixture of those gases with each other and with oxygen to a regulated temperature, the correct analysis of such mixtures might probably be accomplished. Mixtures of two or more of the combustible gases were therefore exposed, in contact with oxygen gas and the platinum sponge, in tubes bent into the shape of retorts, which were immersed in a mercurial bath. This bath was gradually heated to the required temperatures, and by proper management of the source of heat was prevented from rising above that degree.

1st.

1st. By subjecting 25 measures of carbonic oxide, 15 of olefiant gas, and 57 of oxygen, in contact with the sponge, to a heat which was not allowed to exceed 350° Fahrenheit till the diminution of volume ceased, all the carbonic oxide was converted into carbonic acid, and the olefiant gas remained in its original volume.

2nd. By exposing in a similar manner 20 measures of carbonic oxide, 21 of carburetted hydrogen, and 36 of oxygen, to a temperature below 400° Fahrenheit, the carbonic oxide was entirely acidified; and on washing out the carbonic acid by liquid potash, the carburetted hydrogen was found unaltered, mixed with the redundant oxygen.

3rd. A mixture of 10 measures of olefiant gas, 10 of carburetted hydrogen, and 58 of oxygen, being heated in contact with the sponge to 510° Fahrenheit, the olefiant gas was silently but entirely changed into carbonic acid, while the carburetted hydrogen was not at all acted upon.

4th. By acting with the sponge upon 42 measures of carburetted hydrogen, 22 of carbonic oxide, 22 of hydrogen, and 28 of oxygen, first at a temperature of 340° Fahrenheit, which was raised gradually to 480°, all the carbonic oxide was changed into carbonic acid, and all the hydrogen into water; but the carburetted hydrogen remained undiminished in quantity, and was found, after removing the carbonic acid, mixed only with the redundant oxygen. In this experiment the diminution of volume had continued some time before there was any perceptible formation of water, the attraction of carbonic oxide for oxygen appearing to prevail over that of hydrogen. The same precedency in the formation of carbonic acid is always apparent when carbonic oxide and hydrogen, mixed even with oxygen enough to saturate both gases, are raised to 350° Fahrenheit.

By thus carefully regulating the temperature of the mercurial bath, the action of oxygen upon several gases (carbonic oxide, olefiant, and carburetted hydrogen, for example,) may be made to take place in succession; and by removing the carbonic acid formed at each operation, it may be ascertained how much of each of the two first gases has been decomposed. The carburetted hydrogen indeed always remains unchanged, and its quantity must be determined by firing it with oxygen by the electric spark. If hydrogen also be present, it is difficult to prevent the olefiant gas from being partially acted upon; but this is of little consequence, as I had shown that it is easy to remove that gas in the first instance by chlorine. It may be remarked, that this method of operating on the aëriform compounds of charcoal gives more accurate results than rapid

rapid combustion by the electric spark, being never attended with that precipitation of charcoal which is often observed when the gases are exploded with oxygen. A regulated temperature also effects the analysis of such mixtures much more correctly than the action of the sponge or balls, because in the latter case the heat produced is uncertain; and though sometimes adequate to the effect, yet there is always a risk that it may exceed or fall short of that degree which is required for the successful result of the analytic process.

From the facts which have been stated, I derive a method of obtaining carburetted hydrogen gas perfectly free from olefiant gas, hydrogen, and carbonic oxide, and mixed only with a little oxygen, which, had it been necessary to my purpose, might also have been separated. The early product of the distillation of pit-coal was washed with a watery solution of chlorine, and afterwards with liquid potash, to remove a little chlorine that arose into the gas from the solution. The residuary gas was next heated with one-fourth of its volume of oxygen at the temperature of 350° Fahrenheit, in contact with the sponge, which converted the carbonic oxide into carbonic acid, and the hydrogen into water. The carbonic acid being removed by liquid potash, there remained only the carburetted hydrogen, the redundant oxygen, and a very minute quantity of nitrogen introduced by the latter gas. Hitherto I have prepared this gas only in a small quantity; but it would be easy to extend the scale of the operation, and to remove the excess of oxygen by obvious methods.

Section III.

Application of the Facts to the Analysis of Mixtures of the combustible Gases in unknown Proportions.

At an early period of the investigation described in the first section, I proceeded to apply the facts of which I was then possessed, to the analysis of a mixture of gases in unknown proportions. For this purpose I caused a quantity of gas to be collected from coal, by continuing the application of heat to the retorts two hours beyond the usual period and receiving the gas into a separate vessel. Gas of this quality was purposely chosen, because from former experience I expected it to contain free hydrogen, carbonic oxide, and carburetted hydrogen, but no olefiant gas; the production of which is confined to the early stages of the progress. After washing it therefore with liquid potash to remove a little carbonic acid, and ascertaining its specific gravity, when thus washed, to be

308,

308, I proceeded at once to subject it to the new method of analysis.

Having ascertained by a previous experiment with Volta's eudiometer that 10 volumes of the gas required for saturation 9 volumes of oxygen, I mixed 43 measures with 43 of oxygen (=41 pure), and passed a platinum ball, which had been recently heated, into the mixture. An immediate diminution of volume took place, attended with a production of heat and formation of moisture. The residuary gas, cooled to the temperature of the atmosphere, measured 43·5 volumes. Of these, 4·5 were absorbed by liquid potash, indicating 4·5 carbonic acid, equivalent to 4·5 carbonic oxide: the rest, being fired in a Volta's eudiometer, with an additional quantity of oxygen, gave 11 volumes of carbonic acid; the diminution being 22 volumes, and the oxygen consumed 22 also—circumstances which prove that 11 volumes of carburetted hydrogen were consumed by this rapid combustion. But of the loss of volume first observed (viz. 86—43·5=42·5), 2·25 are due to the carbonic acid formed; and deducting this from 42·5 we have 40·25, which are due to the oxygen and hydrogen, converted into water; and 40·25 × $\frac{2}{3}$ = 26·8 shows the hydrogen in the original gas. But the sum of these numbers (26·8 + 4·5 + 11) being less by 0·7 than the volume of gas submitted to analysis, we may safely consider that fraction of a measure to have been nitrogen. The composition then of the mixture will stand in volumes as follows:

Hydrogen	26·8	62·32
Carbonic oxide	4·5	10·50
Carburetted hydrogen	11·0	25·56
Nitrogen	0·7	1·62
	43·0	100·0

On calculating what should be the specific gravity of a mixture of gases in the above proportions, it was found to be 303; which coincides as nearly as can be expected with the actual specific gravity of the gas submitted to analysis, viz. 308. To place the correctness of the results beyond question, I mingled the gases in the above proportions, and acted on the artificial mixture in the same manner as on the original gas; when I had the satisfaction to find that the analytical process again gave the true volumes with the most perfect correctness for the hydrogen and carbonic oxide, and within the fraction of a measure for the carburetted hydrogen. Notwithstanding this successful result, which was twice obtained, I should still prefer, for the reason which has been stated, having recourse to a temperature carefully regulated, for the analysis of similar mixtures, in all cases where the hydrogen is in

moderate

moderate proportion, and where great accuracy is desirable. Whenever (it may again be remarked) olefiant gas is present in a mixture, it should always be removed by chlorine before proceeding to expose the mixture to the agency of the spongy metal.

It can scarcely be necessary to enter into further details respecting methods of analysis, the application of which to particular cases must be sufficiently obvious from the experiments which have been described on artificial mixtures. The apparatus required is extremely simple, consisting, when the balls are employed, of graduated tubes of a diameter between 0·3 and 0·6 of an inch, or, when an increased temperature is used, of tubes bent into the shape of retorts, of a diameter varying with the quantity of gas to be submitted to experiment, which may be from half a cubic inch to a cubic inch or more. These when in use may be immersed in a small iron cistern containing mercury, and provided with a cover in which are two holes, one for the tube and the other for the stem of a thermometer, the degrees of which are best engraved on the glass.

By means of these improved modes of analysis, I have already obtained some interesting illustrations of the nature of the gases from coal and from oil. I reserve, however, the communication of them till I have had an opportunity of pursuing the inquiry to a greater extent, and especially of satisfying myself respecting the exact nature of the compound of charcoal and hydrogen, discovered some years ago by Mr. Dalton, in oil gas and coal gas, which agrees with olefiant gas in being condensible by chlorine, but differs from it in affording more carbonic acid and consuming more oxygen.

XLV. *Remarks respecting Mr.* VANUXEM'S *Memoir on a Fused Product, erroneously identified with the Fused Carbon of Professor* SILLIMAN; *with some additional Facts and Observations.** *By Dr.* ROBERT HARE.

To Alexander Tilloch and R. Taylor, Esqs. Editors of the Philosophical Magazine and Journal.

Gentlemen,

OBSERVING that a memoir by Professor Vanuxem, founded on an unlucky mistake, has been re-published in your Magazine (vol. lxiv. p. 467), I beg that in justice to Prof. Silliman you will, at as early a period as possible, allow the remarks

* Published in the Philadelphia Medical Journal, and Silliman's Journal in the summer of 1824.

 which

which I have made in reply to that memoir to appear before the public in the same channel. I inclose in this letter a copy of them. I am, gentlemen, respectfully yours, &c.

Philadelphia, March 15, 1825. ROBERT HARE.

Professor Silliman, about two years ago, published an account of some phænomena observed during the ignition of pieces of charcoal by a galvanic deflagrator, the poles of which they had been severally employed to terminate. On the charcoal attached to the positive pole a projection was observed to ensue—in the other, a corresponding concavity. The projection he supposed to consist of carbon, fused, volatilized, and transferred from the charcoal of the opposite pole, where the concavity was discovered.

In a late number of the Journal of the Academy of Natural Sciences of Philadelphia, Mr. Lardner Vanuxem communicates his observations on a supposed specimen of fused charcoal, sent to Professor Cooper by Dr. Macneven of New York, which appears to have been iron—and the author appears to have received, and evidently intends to convey, the impression, that the substances considered as fused or volatilized carbon by Professor Silliman must have been similarly constituted.

Mr. Vanuxem, speaking of the mass which he has examined, informs us, that—"It consisted of one large and one small globule, connected together by a thread, or thin bar, of the same material, and resembled a double-headed shot."

And again he says:—"It was then put into an agate mortar, pressed and struck with considerable force—finding it yielded without breaking, and observing that it received a polish, it was examined and found to resemble iron. To confirm the analogy, it was next tried with a file, which acted upon it as it would on soft steel or iron—after this it was subjected to a magnet, to which it readily attached itself—and lastly, with a hammer: by its great malleability, conjoined with the characters just mentioned, it proved its identity with iron."

He moreover states, that the substance in question was attacked by nitric acid, and afterwards was chiefly taken up by muriatic acid, whence an hydrated peroxide of iron was precipitated by ammonia.

On reading this account of the substance examined by Mr. Vanuxem, it was evident to me that it had not the slightest resemblance to those which Professor Silliman had described as fused carbon. A product which I had myself obtained, and which corresponded perfectly with his description, had been preserved in a glass tube. This substance crumbled when subjected to pressure—acquired no polish by hammering

or

or filing—was utterly devoid of attraction for the magnet—was not acted upon by nitric acid—nor did muriatic acid, which had been digested on it, yield any oxide of iron, or give any other indication of that metal.

These observations were made by my friend Mr. G. T. Bowen, under my inspection. Mr. Bowen assisted Professor Silliman at the time when he first made his observations on the fusion of carbon. On perusing Mr. Vanuxem's memoir, Mr. Bowen was no less convinced than myself that there had been a mistake, which, considered as the foundation of a broad and unreserved, though indirect, contradiction given to Professor Silliman's representations, is really unfortunate.

I do not feel authorised to decide whether the substance analysed by Mr. Vanuxem was that which Dr. Macneven forwarded. By *oversight*, one minute portion of matter may be exchanged for another as easily as mistaken—but supposing that the mistake originated with Dr. Macneven, it should be recollected that he did not act under the idea of any serious responsibility. He was writing to a friend, not controverting the conclusions of a skilful chemist.

It was in January last that Dr. Macneven first operated with a deflagrator. I then sent him the first he ever had. Notwithstanding his well-known accuracy, in cases where his opportunities of observation are duly great, it is not unaccountable that amid the hurry of his lectures and his practice he should have mistaken a globule of iron for a specimen of fused carbon. But considering Professor Silliman's great experience and skill as a mineralogist and chemist, and his having operated with the deflagrator for nearly a year before his memoir on the fusion of charcoal was published, it ought not to have been so readily supposed that in scrutinizing the substances which he had obtained, *with a view to communicate the result to the public*, any *advantageous* employment of the magnet, the hammer, the file, or the mineral acids, had been omitted*.

It is true, as Mr. Vanuxem observes, that the incineration of charcoal proves it to contain impurities—but those impuri-

* It appears from Professor Silliman's memoir (vol. v. p. 363, American Journal of Science), that he did employ boiling sulphuric and boiling nitric acid; and moreover, it is evident that the products which he represented as fused carbon could not have been iron, both on account of their habitudes with these acids, and on account of their disappearance when subjected to the solar focus in oxygen gas. Of course no "*advantageous*" application of the magnet could have been made. In examining the globules produced upon plumbago, when exposed to the deflagrator, it will be found that Professor Silliman did resort to the magnet. Iron being a constituent of plumbago, it was in that case rational to expect that the globules might be magnetic. The magnet was also employed by him in testing the globules procured from anthracite, by means of the deflagrator.

ties

ties are well known to be earth or alkali, with a very minute portion of iron, if any. These facts, thus cited by him, are therefore irreconcilable with his inference, that a piece of charcoal of about an inch in length, and less than a quarter of an inch in thickness, could, instantaneously, at its point, form a projection of matter almost solely ferruginous.

I will take this opportunity of observing, that the most interesting phænomena observed by Professor Silliman do not to me appear to be dependent for their importance on the nature of the projection which arises on carbon when forming the negative pole of the deflagrator. That such an excrescence arises, and that a corresponding crater or pit takes place in the charcoal on the opposite pole, are the facts which principally interest me.

I should have done more to prepare myself for the solution of the doubts which have been excited respecting some of the observations of Professor Silliman, had not my eyes been so much affected by a powerful deflagrator, made about two years ago, as to be distressed by any subsequent employment of them in the same way.

From a cursory observation made last winter, I was led to suppose the light of the deflagrator to be equal to that of sixteen hundred candle flames, condensed within a space no larger than that usually occupied by one.

Since the above was written, in trying a deflagrator made for Professor Nott, the operator had his eyes so much affected as to be bloodshot next day*.

By means of the same deflagrator a specimen of the fused or volatilized charcoal was obtained. This did not prove to be magnetic. Instead of being malleable, or susceptible of a metallic polish, it was friable, and the fragments were without brilliancy. Seen by the aid of a powerful microscope, before it was broken, it was, both in colour and shape, exactly like the depositions or concretions of carbon, which have been formed in some instances during the gas-light process.

P. S. It is remarkable that, since the observation last mentioned was made, I have found that Mr. Conybeare, in some speculations on the concretions of carbon, noticed in gas-light cylinders, infers, that they may have some analogy with the products described by Professor Silliman as fused carbon.

* I have considered it proper to dwell on the injury thus sustained by the eyes, that others may by due caution, in the first instance, avoid the evil. The deepest green spectacles should be used, putting two glasses together, when one is not enough. Persons not provided with proper spectacles may use a piece of card, or paper, pierced with some fine holes. Through a hole made by a pin the phænomena may be viewed satisfactorily.

XLVI. *On*

XLVI. *On Plans in Relief.* *By* A Correspondent.

To the Editor of the Philosophical Magazine and Journal.

Sir,

SUCH of your readers as have seen the plan of the "Swiss Mountains" in relief, with a similar plan of the city of "Edinburgh," will readily admit their interest as a matter of taste, and their political and geological value. The "Pyramids" with the sphynx and neighbouring country may be seen in the royal library at Paris; and I believe that a plan of "Gibraltar" in relief has been executed with great minuteness. Evelyn, the author of the Sylva, mentions in an early part of the Philosophical Transactions that he saw similar plans on the continent, and particularizes an island which gave him great satisfaction.

The want of such plans has been felt in the original projection of important drainages, roads, canals, rail-ways, and all other works depending upon levels. *Shading* and *sections* are imperfect substitutes; and an actual plan in relief, except of a mountainous country, is almost useless from the small proportion which the elevations bear to the horizontal plane. But why should not plans in relief be formed, such that the altitude of any point above the horizontal plane shall always be increased in a *fixed ratio* to the *true* altitude? If the altitude, for instance, be increased (n) times, it will in all parts of the plan bear this fixed proportion to the base line, and will truly exhibit the *direction* of the inclination, and its *comparative* magnitude. No ordinary map whatever is mathematically true; when the lines in longitude are correct, the latitudinal distances must be incorrect, and conversely: the various projections have always been formed on some conventional principle, never accurately exhibiting the true horizontal distance.

To the artist I leave the mode of multiplying such plans in plaster, copper, or otherwise; and the contrivances necessary for forming a series into a volume, folded up like a backgammon board.

Septimus.

XLVII. *On the Locality of Rain.* *By Mr.* James Stockton.

To the Editor of the Philosophical Magazine and Journal.

Sir,

IN Dr. Burney's valuable paper inserted in your last Magazine, relative to the great difference of rain caught at different places in the British isles, he makes mention of this place, and states

states the annual quantity falling here at 40 inches. As the last three years have been excessively wet, the learned Doctor may have probably imagined from the perusal of my reports for those periods, that our average quantity for a longer consecutive series of years is equal to this amount. I respectfully beg leave to request your insertion of the following, which are extracted from the commencement of the Meteorological Register, to the close of 1824.

		Inches.		Inches.		Inches.
Amount of rain in	1817,	28·02...	1818,	32·47...	1819,	30·85
————	1820,	29·43...	1821,	28·96...	1822,	36·10
————	1823,	42·40...	1824,	36·74...	Mean	33·121

The situation of this place ought also to be taken into account. Standing, for the greater part in a valley, and the other on a gentle acclivity, and each bordering on the river Derwent, which is bounded by the Yorkshire wolds from east to south, and by the moors and the German Ocean from west to north, with the numerous and thickly-wooded plantations, &c. at Castle-Howard, the seat of the Earl of Carlisle, about five miles distance to the south and south-west of the town, the difference in the above annual amounts will not excite so much surprise as might at the first view be imagined.

Mr. Howard remarks in his Climate of London (vol. ii. tab. 71): "In travelling at different intervals during this month (July) between London and Folkestone, I have observed that the showers, in great measure, avoided the high chalky tracts, and followed the course of the rivers and moist valleys. The reverse distribution sometimes takes place."

During the last eight, and several preceding years, in nearly all seasons, but more especially in the rainy periods of that of the estival, I have frequently made the same observation. Whenever the thunder and other storms followed the course of the Derwent through the valleys adjoining, and were unattracted by the woods, &c. at Castle-Howard, we uniformly experienced a more than plentiful precipitation; but when the reverse occurred, and the clouds composing the storm separated and took different directions, from south to east and west to north, we almost invariably escaped the showers.

In mentioning the above, Dr. Burney's observations as to the diminution of the bulk of rain at New Malton, compared with that at Kendal, appear to me to be more strikingly confirmed. I am, sir, yours, &c.

New Malton, April 19, 1825. JAS. STOCKTON.

XLVIII.

XLVIII. *On new Terms in Geometry.* By Mr. M. Smith.

To the Editor of the Philosophical Magazine and Journal.

Sir,

I HAVE long thought that one great impediment to the study of Solid Geometry consists in the defective state of the present nomenclature adopted in that branch of the science. This may be exemplified in the fact that several of the most simple figures have no names whatever allotted to them, and can therefore only be expressed by a circumlocution; such for instance is the figure of a brick, a box, or an apartment of a house, which, as the nomenclature now stands, can only be described as a *rectangular parallelopiped.* Now as the six planes which bound this figure are all rectangles, I would propose simply to call it a *rectagon.* Again, as a rectagon all the sides of which are squares is denominated a cube, I propose that where two opposite sides (only) are squares, it should be called a *cuboid;* and this may be denominated oblong or oblate, according as the inscribed spheroid is oblong or oblate.

Another figure much used in Spherics is that which bears the same analogy (though not the same proportion) to the sphere as the quadrant does to the circle,—perhaps the term *spherant* would well apply: it might be defined the eighth part of a sphere, produced by the intersection of three planes at right angles with each other passing through the centre of the sphere.

For the convenience of expressing the area of spherical triangles, or other portions of a spherical surface, I would propose to divide the curve surface of the spherant into 90, 5400, and 324000 equal parts, to each of which divisions appropriate names should be given. The advantage of this would be, that in any spherical triangle the excess of the three angles above 180 degrees would be the measure of the area, merely changing the terms degrees, minutes, and seconds into the new terms. By this arrangement the measure of solid angles might also be expressed with much facility.

Those geometrical figures which bear the same relation to the sphere as the sector and segment do to the circle, ought also to be furnished with names: the term *sphericon* would perhaps aptly designate the former, which is in fact formed by a sphere and a cone; for the latter, no name at present occurs to me with which I am quite satisfied.

In Astronomy also some new terms might be advantageously applied,—such as *zenitude* for zenith distance; and I would also propose that in stating the azimuth of any celestial body, it might be denominated NE., NW., SE., or SW., according to the cardinal points between which it may be situated. Thus

instead of saying azimuth S. 47° 20′ E. as is usually done, I would express it thus, azimuth 47° 20′ SE.

While on the subject of Astronomy, let me notice the strange anomaly that one planet of the solar system should have a name which does not harmonize with those of the others, all of which are taken from the heathen mythology; the consequence is, that it is named differently by different authors, being called in turn the Georgium Sidus, the Georgian, Uranus, and Herschel. Now as this planet was a British discovery, and made at a period when the naval achievements of England might justly entitle her to assume that the god of the ocean presided over her destiny, ought she not with gratitude believe that the deity, which then for the first time revealed himself to her admiration, could be no other than *Neptune?* Should this name be thought appropriate, the astronomical character of the planet might be made to resemble that of Mars, merely substituting the trident for the spear.

I have thrown out these few hints sir, in the hope, that the subject will attract the notice of some abler pen, or that it will be taken up by some scientific body of men, whose authority would ensure currency to such new terms as they might decide upon.

I am, sir, yours, &c.

April 25, 1825. M. SMITH.

XLIX. *Mr.* STURGEON *on the Cause of the Earth's Motion: in reply to Mr.* BEVAN.

WE have received a communication from Mr. Sturgeon, relative to Mr. Bevan's letter in our last, from which we give an extract.

"I feel no astonishment at the similarity of opinion entertained by Mr. W. Herapath and myself respecting the cause of the rotary motion of the earth; for the same idea has struck several gentlemen to whom I have exhibited electro-magnetic experiments.

"Priority of *opinion* would at all times be a difficult task to discover; but for priority in the support of opinion, when accomplished by the test of successful experiment, I presume that Mr. W. Herapath's friend cannot in the present case put in any just claim. If, however, a more early date than that of my last paper, which I consider satisfactory on the subject, be required, I must refer those gentlemen to another, dated Feb. 16th, 1824, in the postscript of which will be found something relative to my rotating sphere. I am not aware that any such experiments have yet been accomplished by

by Mr. Herapath; neither have I any reason to believe, that my attempt to rotate either the Galvanic Sphere, or the Magnet on its axis by the influence of two electric currents, could have proved successful, had I entertained the views of the principles of electro-magnetism which he appears to hold."

L. *Notices respecting New Books.*

An Explanatory Dictionary of the Apparatus and Instruments employed in the various Operations of Philosophical and Experimental Chemistry. With 17 Quarto Copper Plates. By a Practical Chemist. London, 1824. 8vo.

THIS is the first work of the kind that has yet appeared, and as such is perhaps as complete as could have been expected. It includes descriptions of some obsolete and nearly useless apparatus, and omits a few others which are important; but on the whole is calculated to be a very useful laboratory companion. The plates are very good. We hope soon to see a second edition; when we doubt not the author will take the opportunity of rendering his work more complete, and thereby still more acceptable than at present to students who desire to become "practical chemists."

A Popular Explanation of the Elements and General Laws of Chemistry. By Walter Weldon. London, 1825. 8vo.

This work, we are informed in the preface, is intended to present a general view of the laws and principal phænomena of chemistry, for the use of "the laborious mechanic, the amateur, and the female reader," who, in their study of chemistry (having other professions to pursue or other avocations to follow), are unable, or unwilling, to spend much time and labour in the acquisition of a general knowledge of the science, to them, for the most part, more ornamental or amusing than useful.

For this purpose Mr. Weldon has included in his work such a detail of facts, he states, as is sufficient to exemplify and explain the principles of the science, using the most explicit and popular terms, avoiding mathematical, and, as far as possible, complex arithmetical calculations, and controversial points.

The work is altogether a compilation, and contains no original views that require notice. We do not find in it any explanations of chemical phænomena that are particularly luminous; but, on the other hand, we have not met with any serious errors: we think the work may be useful to those for whose perusal it is intended.

Lunar and Horary Tables for new and concise Methods of performing the Calculations necessary for ascertaining the Longitude by Lunar Observations or Chronometers; with an Appendix, containing directions for acquiring a knowledge of the principal Fixed Stars. By David Thomson, inventor of the Longitude Scale.—Kingsbury & Co. London; Oliver and Boyd, Edinburgh. Price 10*s*.

These Tables for clearing the lunar distances from the effects of parallax and refraction, are designed to facilitate the computations necessary for ascertaining the longitude; and they certainly appear to us to be better adapted to that object than any of the various methods with which we are acquainted: they are at once concise and simple; little is left to the knowledge or discretion of the calculator; every thing is ready performed to his hand; neither too much is given nor too little;—and, what is of some importance, all contained within a thin octavo volume, at the price of ten shillings,—so that it will come within the reach of seamen in general. One particular excellence, which is seldom to be met with in methods founded on the same principle, and which must be highly approved by navigators, is, that there is *no distinction of cases*,—all the corrections on account of parallax and refraction are *additive*. We have not been able to enter into a detailed investigation of the tables, nor has the author stated the principles of their construction and application.

Prof. Schumacher, with a laudable zeal for the promotion of science and its application to objects of utility, has in his Ephemeris extended our means of comparison by inserting the distances of the different planets from the moon; and we think it important that no encouragement should be wanting to induce the navigator on all occasions to practise the lunar observations as the best, because the *only certain* means of ascertaining the place of his ship: any method which diminishes the number of mechanical operations requisite to be performed in the reduction of an observation, without the sacrifice of simplicity will be extremely serviceable towards bringing this much neglected method into more general practice at sea.

Recently published.

Memoirs of Moses Mendelsohn, the Jewish Philosopher; including the celebrated correspondence on the Christian Religion with J. C. Lavater, Minister of Zurich. By M. Samuels. Longman and Co.

The Century of Inventions of the Marquis of Worcester. From the original MS. With historical and explanatory Notes and a Biographical Memoir. By Charles F. Partington.

COL-

COLLECTION OF NATURAL HISTORY.

A valuable and interesting collection of insects and birds, made and preserved with the greatest care by an eminent German entomologist, will be for sale at Mr. Thomas's, of King-street, Covent Garden, in the course of May.

ANALYSIS OF PERIODICAL WORKS ON NATURAL HISTORY.

Curtis's Botanical Magazine. No. 455, 456, 457.

(Continued from p. 52.)

Pl. 2531. *Crinum arenarium β.* Coast of Australia, and isles adjacent.—*Pergularia sanguinolenta* "foliis ovato-lanceolatis glaberrimis petiolatis, cymis multifloris folio brevioribus, corollæ laciniis acuminatis obtusis, succo sanguineo."—*Hamelia patens.*—*Cyrtanthus striatus.* *Paliurus virgatus*, Don *Prodr. Fl. Nepal.*—*Clerodendrum macrophyllum* "foliis lato-ovatis acuminatis serratis subsessilibus subtùs tomentosis, floribus paniculatis, calycibus 5-dentatis, corollis labiatis."

Pl. 2537. *Zephyranthes rosea.* Mr. Herbert corrects his generic character given in the Botanical Register, stating that the flowers are suberect, but not vertical. as there stated.—*Pancratium Zeylanicum:* this genus is here limited to the true congeners of *maritimum*, on which it was founded, in Europe, Asia, and Africa. Some animadversions are also made on the editor of the Botanical Register, who has not adopted the genera *Carpodetes, Leperiza*, and *Stenomesson* proposed in the Appendix of Bot. Mag., but comprehended them in a new genus, *Chrysophiala*, under which name he has figured *flavum* (*Pancratium* of Ruiz and Pavon).—*Gloriosa virescens* "foliis cirrhiferis, pedunculis pendulis, petalis unguiculatis apice undulatis." The plant described by Lamarck as a doubtful variety of *G. superba* is said to answer very well to this subject, which is from Mosambique, the other from Senegal.—*Goodyera pubescens β.* minor. It differs from *α* in size only, except that the leaves are less oblong, and with more obscure markings.—*Lavatera hispida.*—*Phlomis lunarifolia β.* M. Lagasca considers this as a distinct species, and proposes to name it after Dr. Russell, who has figured it in his history of Aleppo.—*Caladium bicolor;* given here by mistake, having already been figured in this work under its former name, *Arum*, from which genus it has been separated by Ventenat.—*Malva abutiloides.*

Pl. 2545. *Aristolochia labiosa.*—*Solidago lanceolata*, now first figured from a plant compared with the specimens in the Banksian herbarium.—*Solanum pyracanthum β.* Duval, in his Monograph, remarks that the peduncles and calyxes are sometimes very thorny, and sometimes are quite without thorns: in this figure these parts are unarmed.—*Scutellaria altissima:* stated to be rather a biennial than a perennial by Mr. Denson, curator of the botanic garden at Bury St. Edmond's, whence the specimen was received.—*Berberis aristata:* "Although we wish that DeCandolle had been contented with the name of *Chitria*, given to this species by its discoverer Dr. Hamilton (late Buchanan); yet as that name had not then been published, the learned Professor was at liberty to apply one that seemed to him more appropriate, and *aristata* being the first published name ought to have been adopted by succeeding writers; we hope, therefore, that we shall lessen rather than increase confusion by preferring it; especially as the Systema Naturalis Regni Vegetabilis is a work that cannot fail to be found in the hands of every botanist, and to be considered of the first authority. Of this inestimable work only 2 volumes have as yet appeared, and it is at present suspended, to give way to the Prodromus, the nature of which allows of its being carried on with greater rapidity. And if it happen that the great

great work should not be resumed, these volumes will bear ample testimony to the industry, learning, and botanical skill of the author."—*Lobelia Tupa.* "This plant differs altogether from our *L. gigantea*, the *Tupa* of the Hortus Kewensis."

Curtis's British Entomology. Nos. 15 and 16.

Pl. 59. *Cossonus Tardii*, a new species discovered in Ireland, never before noticed, and greatly exceeding in size the other species found in this country.—*Cossus ligniperda*, a fine variety of this well-known insect with its larva (celebrated amongst the Romans as a great delicacy) is here represented.—*Anthidium Manicatum:* this curious bee has been noticed by most authors; and we find very amusing and interesting accounts of it from the Rev. G. White's Naturalist's Calendar, as well as from the pen of the Rev. W. Kirby.—*Dolichopeza sylvicola*, a new genus of a nondescript *Tipula*, found by the author in the New Forest.—*Acilius cinereus:* both sexes of this fine insect, new to Britain, are given in this plate.—*Eupithesia Linariata:* this pretty and rare little moth, although well known as an inhabitant of Britain, has never before been figured in any of our own works.—*Hylotoma Stephensii*, a new species of *Tenthredo*, named after its first discoverer, who took it in Kent; it was unknown to Klug, and Le Peletier St. Fargeau is only acquainted with it by Dr. Leach's description.—*Helcomyza ustulata*, an insect new to Britain, named from the manuscript of M. Meigen.

Zoological Journal.

No. III. of this work contains An Inquiry into the natural affinities of the *Laniadæ*, by Mr. Swainson.—Sketches in Ornithology (on the *Falconidæ*), by Mr. Vigors.—Continuation of Mr. French's second Essay on Instinct. —Continuation of Mr. Gray's Monograph on the *Cypræidæ*.—Mr. Bennet's Translation of Chabrier on the Thorax and Flight of Insects; and a description of a new species of *Buccinum*, by the same naturalist.—M. E. Geoffroy Saint-Hilaire on the vestiges of placental organization in *Didelphis Virginiana*.—Mr. Gray on the Structure of *Pholades*,—and Descriptions of some rare, interesting, or hitherto uncharacterized subjects of Zoology, by Mr. Vigors; with plates by Mr. J. D. C. Sowerby.—Reviews of Zoological Works, Proceedings of Societies, &c. &c.

Among the species described in the last article is a new and beautiful Parroquet, lately brought from the island of Toohooteterooha, in the Pacific Ocean, about a day's sail from Otaheite; and thus characterized by Mr. Vigors: "*Psittacula Kuhlii.* P. flavo-viridis, gutture, genis, pectore, abdomineque coccineis, crista occipitali, fasciâque abdominali purpureis, rostro pedibusque rubris."

No. IV. (concluding the first volume) contains Some Remarks on the nomenclature of the *Gryllina* of MacLeay, with the characters of a new genus in that tribe, by the Rev. Mr. Kirby.—An Account of the unexampled devastations committed by Field-mice in the Forest of Dean, and in the New Forest, in the years 1813 and 1814, by the late Lord Glenbervie.—Remarks on the devastation occasioned by the *Hylobius Abietis* in fir plantations, by Mr. W. S. MacLeay.—Observations on the British *Tipulidæ*, together with descriptions of the British species of *Culex* and *Anopheles*, by Mr. Stephens.—Mr. Bell on a new species of Lizard.—Mr. Burchell on *Malaconotus atro-coccineus*.—Mr. Swainson on some new birds from Australasia.—Mr. Broderip on the manners of the Toucan.—Continuation of Mr. Gray's Monograph on the *Cypræidæ*.—M. de Ferussac and Mr. G. B. Sowerby on *Ætheria*.—Descriptions of new subjects of Zoology, by Mr. Vigors.—Description of the Rimau-Dahan of Sumatra, a new species of

of *Felis* discovered by Sir T. S. Raffles; by Dr. Horsfield.—Descriptions of some new Brazilian species of the family of *Laniadæ*, by Mr. Such.—Description of the *Vespertilio Pygmæus*, a new species recently discovered in Devonshire by Dr. Leach.—Notices of Zoological Books, &c.—Dr. Horsefield's paper on the Rimau-Dahan, or, as M. Temminck has denominated it, *Felis Macrocelis*, is one of the most interesting articles in the number; he gives the following specific character of the animal. "Felis grisea, maculis nigris: humeralibus maximis transversis; lateralibus obliquis subcoadunatis vel intervallis angustioribus divisis plagis angulatis rotundatisve rarius ocellatis; omnibus marginibus posterioribus saturatioribus, lineis cervice dorsoque summo duabus parallelis: collo utrinque duabus superiore continua inferiore interrupta, pedibus validis, podiis amplis robustis, cauda longissima incrassata lanuginosa."

LI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 14.—THE reading was commenced of "A Monograph on Egyptian Mummies, with observations on the Art of Embalming among the Ancient Egyptians;" by A. B. Granville, M.D. F.R.S.

April 21.—The reading of Dr. Granville's paper was continued.

April 28.—The reading of Dr. Granville's paper was concluded.

THE LINNÆAN SOCIETY.

March 15.—Read a paper from R. A. Salisbury, Esq. F.R.S., F.L.S., &c., on the *Trichomanes elegans* of Mr. Rudge's *Plantæ Guianæ*. It appears that M. Bory de St. Vincent asserts in the 6th vol. of the *Dictionnaire classique d'Histoire Naturelle*, under the article *Fougere*, that Mr. Rudge's plant, t. 35, is composed of two species of different genera, one of which M. Bory proposes as a *Feea*, and the other as constituting a new genus, under the name of *Hymenostachys*. Mr. Salisbury how ever insists, that M. Bory's assertions are devoid of any foundation, and he attributes his criticisms to an ignorance of the Latin language. In confirmation of this opinion, Mr. S. exhibited the specimen itself from which the figure had been drawn, that he might afford occular demonstration, that it consisted of one individual.

To corroborate this opinion, he adduces the testimony of Professor Hooker, who in his 52nd plate of his *Exotic Flora*, refers to Mr. Rudge's figure and gives a coloured one of *T. elegans* the involucrum of which contained ripe capsules.

The question being a matter of reference to the Society, the Vice President named Mr. Edward Forster, Mr. Bicheno, and Mr. Menzies to investigate the matter and report thereon in pursuance of a bye-law of the Society.

April

April 5.—A valuable present of stuffed birds and fishes was received from Capt. King, collected by him in his late expedition to explore the north-west coast of New Holland.

The Committee appointed at the preceding meeting made their report relative to Mr. Salisbury's paper on *Trichomanes elegans;* and stated that the plant was represented to have been gathered in Guiana, by M. Martin, and to have been purchased by Mr. Rudge. It belongs to the genus *Trichomanes* of Smith. M. Bory asserts that the spike described as the mature fructification, is of a totally different structure from the others, which are regarded as immature. It appears that Hooker did not doubt the fidelity of Mr. Rudge's plant, though his own figure supports M. Bory's opinion, inasmuch as the fronds there delineated differ from those in Mr. Rudge's figure.

M. Poiret has described, and M. Desveaux has both described and figured, the plant which corresponds with the fructification supposed to be mature. Weber and Mohr have also the same species.

In the Banksian and Mr. Brown's collection, were found several specimens of each of the two plants, alleged by the French author to be confounded, in all stages of fructification. In every instance the Committee found the barren frond of Mr. Rudge's specimen combined with the fructification which he calls the young state; and as constantly the frond, figured by Hooker, with the spike which is said to be mature.

The specimen itself was also subject to their inspection, and upon a minute examination of it, they were satisfied that it was composed of two individuals: They therefore reported that M. Bory appeared to them to be justified in his conclusions.

It was added, that they thought it but justice to Mr. Brown to say, that Mr. Salisbury was correct in stating that M. Bory had fallen into the error of making Mr. Brown adopt Willdenow's arrangement of the Ferns, whereas Mr. Brown's work made its appearance in the same spring, but before Willdenow's, and his arrangement is materially different.

A further portion of Dr. Hamilton's Commentary on the *Hortus Malabaricus* was also read.

April 19.—A continuation of the Rev. Messrs. Sheppard and Whittear's paper on Norfolk and Suffolk birds was read.

GEOLOGICAL SOCIETY.

March 18.—The paper entitled "Observations on the beds of clay, sand and gravel, belonging to the red marl formation of the midland counties, and in the rocks from which they are derived, by the Rev. James Yates, M.G.S. was concluded.

In this communication Mr. Yates enters into some description of

of the rocks which are found *in situ* on the confines of Wales and Shropshire, in order to show that from the disintegration of these rocks, the clay, sand and gravel, of the red marl formation have for the most part been derived. The first line of section which is particularly considered is near the river Dee and Vale Crucis; the second, a line drawn from Oswestry westward to Llansilen, which crosses within the space of five miles the basset edges of all the strata from the new red sandstone to the slate. The author then takes a view of the rocks occurring in the direction of the road from Welchpool to Ludlow. The fourth district then noticed is the vicinity of Church Stretton. Mr. Yates then mentions some particulars of the rock near Bewdley and in the Clent Hills, and the neighbourhood of Dudley, and adds some remarks on the Bromsgrove Lickey, as supplementary to Professor Buckland's paper in the 5th volume of the Society's Transactions.

The range of hills is also described which extends from N.W. to S.E. beside the course of the Coventry canal and the river Anker; and lastly, a district in Leicestershire, a few miles E. from Hinckley, consisting of a coarse-grained crystalline greenstone.

The author then proceeds to show how the strata belonging to the older formations which he has described, may be viewed in connexion with the general physical structure of England; and then points out from what various sources the beds of sand, clay and gravel, of the red marl formation, as well as the superficial debris which is strewed over the midland districts of England, may have originated. Mr. Yates concludes with some remarks on the excavation of valleys, and on some opinions on that subject now generally received among English geologists from which he is inclined to differ.

April 15.—A paper was read entitled, "On a new species of Gyrogonite from the lower fresh-water formation at Whitecliff-bay in the Isle of Wight, with some account of the strata in which it occurs;" by Charles Lyell, Esq. Sec. G.S.

Mr. Lyell describes this species of gyrogonite as very distinct from the three species which have been found in France. The spiral valves form nine rings, each of which are ornamented with a row of tubercles; from which he has given it the name of *Chara tuberculata.* An account is given of the strata of the lower fresh-water formation at Whitecliff-bay in the Isle of Wight, in which this gyrogonite occurs very abundantly. They consist of beds of compact limestone alternating with whitish calcareous marls, and in most of them the casts or shells of various fresh-water univalves are common.

Gyrogonites appear not to have been noticed before in the

 fresh

fresh-water strata on the east side of the Isle of Wight. Those which have been noticed as abounding in the limestone of the lower fresh-water at Garnet Bay are chiefly referable to the *Chara Medicaginula* of the French authors; in that locality fossil stems accompany them whose structure is identical with that of some recent Charæ, as for example *C. hispida.*

The author concludes by observing, that from the remarkable toughness of the integument of their seed-vessel, and from the large proportion of carbonate of lime which they contain in a living state, most of the Charæ are peculiarly adapted for becoming fossil, and that they are accordingly preserved in the recent marls in Scotland, both in a vegetable and a mineralized state, when the other aquatic plants which lived and died in the lakes with them are entirely decomposed, or can no longer be recognised.

An extract of a letter was read from Jer. Van Rausselaer, Esq. on the discovery of the skeleton of a mastodon at New-York; and of the tertiary formation in New Jersey.

In this letter Mr. Rausselaer mentions that in a late expedition which he had made with some friends to examine the geology of the state of New Jersey, they had discovered, disinterred, and afterwards brought to New-York, the skeleton of a mastodon very nearly perfect. They also satisfied themselves that much of the region which lies between the Atlantic and the range of primitive mountains was referable to the tertiary formations, and that the secondary do not make their appearance for some hundreds of miles.

A paper was read, entitled "Account of a fossil crocodile recently discovered in the alum shale near Whitby;" by the Rev. George Young.

Mr. Young describes the osteology of this fossil animal which has been deposited in the museum at Whitby, and of which a drawing accompanied this communication. Its length exceeds 14 feet, and when perfect must have reached 18.

The author mentions that these are not the only remains of the crocodile which have been discovered near Whitby, although they had been generally confounded with those of the *Plesiosaurus;* of which animal, however, as well as of three or four species of the *Icthyosaurus*, undoubted remains occur in the alum shale of Whitby.

HORTICULTURAL SOCIETY.

March 1.—The following papers were read: On the cultivation of the pine-apple; by Mr. William Chartres, corresponding member of the Society.—A description of the vineries of John

John Wright Hulme, Esq.; by James Robert Gowen, Esq., F.H.S.—An account of a method of obtaining very early crops of the grape and fig; by the President.

March 15.—The following papers were read: On the cultivation of the Neapolitan violet; by Mr. Thomas Ashworth, corresponding member of the Society.—Description of a peach-house, and mode of training practised in it; by Mr. William Beattie, F.H.S.

April 5.—His Majesty the King of France and His Imperial Highness the Archduke John of Austria were elected Fellows of the Society.—The silver medal was presented to John Dickson, Esq., of Rio Janeiro, corresponding member of the Society, for various important services rendered by him to the Society by the transmission of plants, and by assistance afforded to its collectors, &c.—The following papers were read: The result of experiments with lime, used in improving the fruit-tree borders of an old garden; by Mr. William Balfour, corresponding member of the Society.

April 19.—His Royal Highness Frederic William Crown-Prince of Prussia was selected a Fellow of the Society; and Rainaud Louis Desfontaines, M.D., Professor of Botany in the Museum of Natural History at Paris, was elected a foreign member in the room of M. André Thouin, deceased.—The following paper was read: On the cultivation of the pine-apple; by Mr. William Greenshields, C.M.H.S.

ASTRONOMICAL SOCIETY.

April 8.—A paper was read "On the results of computations on astronomical observations made at Paramatta, in New South Wales, under the direction of Sir Thomas Brisbane, K.C.B.; and the application thereof to investigate the exactness of observations made in the northern hemisphere. By the Rev. John Brinkley, D.D. F.R.S. &c." Anxious to throw some new light on the subject of the discordance between the north polar distances of the principal fixed stars, as determined by Continental and English astronomers, Dr. Brinkley wrote to Sir Thomas Brisbane, to request His Excellency to make some observations at Paramatta. Sir Thomas immediately commenced the important labour;—and on a series of three months' observations, from November 1823 to February 1824, communicated to this Society as well as to Dr. Brinkley, the Doctor has founded the computations and comparisons which are communicated in this paper.

The sum of the polar distances of a star observed in the two hemispheres ought to be exactly 180° if both are correctly observed. Also, on the hypothesis that the mean refraction is

is the same in both hemispheres, we have an opportunity of ascertaining the united effects of refraction, instead of the difference between the refraction of a star near the pole and of a circumpolar star remote therefrom.

In regard to the distance between the north and south poles, by combining Dr. Brinkley's observations with those of Sir Thomas Brisbane, the result is, that the mean of 141 south polar distances deduced from 141 of his observations, and applied to Dr. Brinkley's north polar distances = 179° 59′ 58″,92 or 1″,08 *in defect*. Dr. Brinkley's refractions were applied to the southern observations, using the *interior* thermometer. The same mean, obtained by using Mr. Bessel's north polar distances and computing by Mr. Bessel's refractions (*Astron. Fundam.*), using the *exterior* thermometer, is 180° 0′ 1″,72 or 1″,72 *in excess*.

1°. Among the observations are some by reflection. These afford us the means of determining the zenith point, and thence the distance between the zenith and polar points, or the co-latitude.

Co-latitude by *Canopus*	56°	11′	8″,63
———————— *Sirius*			9 ,16
———————— *Fomalhaut*			9 ,95
Mean =	56	11	9 ,25
Latitude =	33	48	50 ,75

2°. The results of observations on *both* the solstices of 1822 appear to show the latitude of Paramatta = 33° 48′ 42″. — (No. 37, *Der Ast. Nachrichten.*)

The observations of the Dec. solstice of 1821 give the mean zenith distance of the solstitial point, Jan. 1, 1822, = 10° 21′ 2″,23 —(No. 20, *Der Ast. Nach.*)

The mean obliquity of the ecliptic, taking the mean obliq. of Jan. 1, 1816, = 23° 27′ 49″,21 and secular diminution = 43″	= 23 27 47 ,06
Latitude	33 48 49 ,29

If we use Mr. Bessel's obliquity = 23° 27′ 45″,66 the latitude will be = 33° 48′ 47″,89.

The result of all the observations shows that Dr. Brinkley's *constant* of refraction (57″,72) is as exact as can be desired, when the refractions are computed by the *internal* thermometer; also that, when computed by the *external* thermometer, Mr. Bessel's refractions require no correction worth notice.

A communication was also read from Colonel Beaufoy, inclosing a series of observations of Jupiter's satellites, at Bushey Heath, near Stanmore, between April 1816 and December 1824; and another series of observations of solar and lunar eclipses,

eclipses, and occultations of stars by the Moon, occurring in the same interval of time, from 1816 to 1824 inclusive. The eclipses of Jupiter's satellites are so recorded as to show the mean time at Bushey, mean time at Greenwich, and then the same as exhibited in the Nautical Almanac. The discrepances between the results of observation and the Nautical Almanac are in some cases very considerable. Even with regard to the *first* satellite, the differences sometimes exceed a minute and a half in time; and with regard to the other satellites, the differences exceed 2, 3, 4, and in one case (July 15, 1818) *seven* minutes of time. In this case the discrepance is the same with respect to the *Connaissance des Tems.*—The others the reporter has not had leisure to compare.

The reading of Mr. Atkinson's paper on refraction was also resumed and continued.

ROYAL ACADEMY OF SCIENCES OF PARIS.

Jan. 3, 1825.—M. Poisson was elected vice-president for the year; and M. Chaptal, vice-president for the year preceding, entered upon his office as president for 1825.—M. de Humboldt communicated extracts from various letters he had received from Italy and America.—M. Pelletan, jun. read a note on the galvanic phænomena which he states to accompany acupunctura-tion.—M. Lacroix, in the name of a commission, made a report on the late M. Peyrard's translation of *Apollonius Pergæus:* the Academy expressed its desire that the printing of this work should be facilitated, as well as of the translation of Euclid.—M. Dupuytren commenced reading a memoir on artificial *ani.*—Capt. Poncelet's memoir, on vertical undershot wheels with curved float-boards, was submitted to a commission.

Jan. 10.—Professor Delille, of Montpellier, presented a memoir on the danger of using mushrooms in cookery.—M. Duméril made a report on a memoir on leeches, by MM. Pelletier and Huzard, jun. [see p. 317.]: he also reported on M. de Ferussac's memoir respecting the animal of the genus *Argonauta.*

Jan. 17.—M. Magendie announced that he had ascertained the insensibility of the retina in a female on whom he operated for cataract: the contact of an instrument with that organ did not produce any appreciable sensation: the patient recovered her sight immediately after the operation.—M. de Basterot read his geological description of the tertiary district of the south-west of France, comprising some general remarks on fossil *Mollusca.*—M. Foderà announced that he should shortly

shortly communicate the results of his researches on muscular contraction, on the action of various agents on the nervous system and on muscular fibre, and on the formation of white globules analogous to the white globules of the blood.—M. Collin presented a memoir on the fermentation of sugar.—M. Cauchy presented a new memoir on the integration of linear equations and on the vibrations of elastic rectangular plates.—M. Giron de Buzareingue read the first part of a memoir on the generation of animals.—M. Bailly communicated some statistic researches on intermittent fevers.

Jan. 24.—The Academy received a memoir, by M. Paul Coqueré, on an experiment in acoustics, for discovering the mutual relation and the number of grave harmonic sounds produced by the co-existence of two or more given sounds; and also a memoir from M. Richardot, officer of artillery, on an economical method of erecting paratonnerres.—M. Raspail read an Essay towards a classification of the *Gramineæ*, founded on the physiological study of the characters of this family.—M. Gaymard read some observations on the *Biphores* and on the *Beroes*.—M. Latreille communicated a notice respecting an insect of the genus *Brachycerus*, which is considered as a talisman by the females of the kingdom of Berta.—M. Lassis read a note on the difference of opinion among physicians relative to the entire subject of epidemic disorders.

Jan. 31.—The Academy received from Professor Briot, of Besançon, two memoirs intended to compete jointly for the prize founded by M. de Monthyon: one, On a new forceps; the other, entitled Considerations on the lachrymal ducts, their disorders, and the means of curing them.—M. Croyset, officer of health at Cus, transmitted (for the same purpose) a box containing various instruments.—M. Morin, of Strasburg, presented two pamphlets: one, On oysters, and the means of keeping them fresh; the other, On aërostation.—M. Voisard, of Metz, presented his researches on the determination of the functions of two variables; and Mr. S. Pugh, his considerations on caloric and on light.—M. Bosc, in the name of a commission, made a favourable report on the second part of the History of Lichens by M. Delise.—M. Duméril, in the name of a commission, made a report on the method of curing the Malformation of the anus.—M. Becquerel read some researches on the conducting power of the metals for electricity.—M. Cauchy presented a memoir on a new kind of calculus resembling the infinitesimal calculus.—M. Colin read a memoir on the vinous fermentation.—M. Delapylaie commenced the reading of a memoir on the *Encornet* of the French fishermen.

Feb.

Feb. 7.—M. Olièvier transmitted from Stockholm some theorems relative to the theory of cog-wheels.

M. Magendie communicated an observation confirming his view respecting the so-called olfactory nerve, that it is not the nerve of smell: this was, that a man in whom the anterior part of the brain and the olfactory nerve had been altered or destroyed, still retained the sense of smell.

Feb. 14.—M. Bailly communicated several results of an investigation in which he is engaged, for the purpose of determining whether the births of males and of females indicate any coincidence with physical causes susceptible of being appreciated by our means of observation. He announced a detailed memoir on the subject.—M Dupetit-Thouars made a report on a botanical memoir by M. Raspail.—M. Cauchy communicated a note on the calculus of remainders and on the definite integrals.

Feb. 21.—M. Duméril presented, in the name of the author, some prophylactic and curative views on the yellow fever, extracted from a memoir by M. Fourreau de Beaunegand on the physical and medical topography of Florence.—M. Latreille gave a verbal report relative to a memoir by M. Loiseleur Deslonchamps, on the means of obtaining several crops of silk in a year.—MM. Desfontaines and Mirbel made a favourable report on a memoir by M. Lamouroux relative to the geography of *hydrophytes*: as did MM. Brongniart and Beudant on M. Basterot's memoir respecting the geology of the tertiary basin of the south of France.

Feb. 28.—M. Opoix (who had invented a method of preserving butter fresh) announced that he had brought it to perfection; he presented a sealed vessel of butter six months old, and requested that it might be examined: M. Deyeux was appointed to this examination.—M. Joseph Lowry transmitted a memoir respecting a progressive projection of the northern and southern hemispheres, with three maps on this principle.—A memoir was submitted to the Academy, entitled Perspective geometry, or A new method of representing objects; by an engineer of bridges and causeways.—A report by M. D'Arcet was read on M. Chevreusse's physico-chemical researches on carbon.—M. Geoffroy St. Hilaire read a memoir on the natural affinities of the fossil crocodile of Caen, and on the formation from it of a new genus, under the name of *Teleosaurus*.—M. Civiale read a summary of observations in continuation of his memoir on the lithontriptor, or new mode of destroying stones in the bladder.—M. Cauchy read an analytical memoir on definite integrals taken between imaginary limits.

limits.—M. Marc-Antoine Parseval presented a memoir entitled General theorems on analytic functions.

LII. *Intelligence and Miscellaneous Articles.*

LECTURES FOR THE MECHANICS AT PARIS.

M. CH. DUPIN finished on Saturday, March 26th, the course of lectures on mechanics and geometry which he delivered at the Conservatoire des Arts et Metiers for the instruction of the working classes. More than 500 persons, chiefly of these classes, attended his lectures; and no doubt can be entertained of their utility, for they were listened to with profound attention. The progress of Industry will become incalcuable when she is guided by Science.—*Courier François.*

SCIENTIFIC INSTRUCTION IN FRANCE.

M. Morin, engineer, formerly a pupil in the Polytechnic School, following the good example of M. Dupin, has begun at Nevers a gratuitous course of mechanics and physics applied to the arts. More than thirty persons, already acquainted with arithmetic and the elements of geometry, attend; some of whom have supplied instruments and models of machines, and others have aided the undertaking by subscriptions. At Perigueux a public museum of the minerals of the departments has been begun.

RESEARCHES OF MESSRS. BOUSSINGAULT, RIVERO, AND ROULIN, IN SOUTH AMERICA.

These enterprising travellers have continued their barometric levelling of the Cordilleras, between Merida, the emerald mines of Muzo, and Bogota: they have observed the horary variations of the barometer on the coast, and at the height of 8530 feet: they have analysed an aërolite weighing 1600 pounds, found on the ridge of the Andes: the pigment of *Bignoina chica*, which, similar to indigo, presents however a peculiar vegetable principle: the hot springs of Mariara, from which very pure azotic gas is disengaged: the milk of the cow-tree, which is nutritive, and contains fibrin and wax; the poisonous and extremely irritating principle of the juice of *Stura crepitans*; and finally the confusedly crystallized saline substances which the Indians obtain from the alpine lake of the Andes of Merida, called Laguna del Urao. Messrs. Rivero and Boussingault found the Urao to be composed of 0·39 of carbonic acid, 0·41 of soda, and 0·19 of water: it is a mixture

a mixture of carbonate and bicarbonate of soda, altogether resembling the *trona* of the Natron-lakes of Africa, analysed by Klaproth. These travellers have also communicated to Messrs. Humboldt and Arago, not only two tables of longitudes, but also all the details of astronomical observations (horary angles, circum-meridian heights of the stars and the sun, immersions and emersions of the satellites of Jupiter) from which the results have been deduced. The astronomical observations of M. Rivero and his associates rectify a part of the geography of South America, no point of which had hitherto been determined with precision: namely, 1st, the country comprehended between the Sierra Nevada of Merida, and the lake of Maracaybo and Bogota; 2nd, the course of the Rio Meta, which unites the series of positions of the Oroonoko with that of the Andes of New Grenada, and of the Rio Magdalena.—*Rév. Ency.* Feb. 1825.

HEIGHT OF MOUNT ELBORUS.

The Elborus, Elburus, Elbruz, Elburz, or Alburz, is sometimes called by the natives the *Shat*, or *Shach-Gora;* but, according to Pallas, the Circassians call it *Osha Mashua*, or the Happy Mountain; and the Akases, *Orfi Ipgub.*

The Elborus is the loftiest mountain of the Caucasus, and one of the highest on the globe: it shows two conical summits, one much higher than the other. According to Pallas, this mountain yields in nothing to Mont Blanc in Switzerland. It was measured, some years ago, by Colonel Boutsovskii, who estimated its height at 16,700 Parisian, or 17,785 English feet, above the level of the sea. If this statement be correct, Mount Elborus exceeds Mount Blanc (which is only 15,630) in height more than 2000 feet.—*Travels in Russia by R. Lyall, M.D. &c.*

ANALYSIS OF THE ANIMAL EARTH OF THE CAVE OF KÜHLOCH.

In our sixty-second volume, p. 112, we gave Professor Buckland's account of the remarkable accumulation of the exuviæ of bears in the cave of Kühloch in Franconia. That geologist having transmitted to M. Chevreul for analysis two specimens of the black earth forming the soil of the cavern, taken at different depths, we extract the following particulars from M. Chevreul's memoir on the subject, just published in the Annals of Philosophy.

Mr. Buckland transmitted to me through Mr. Underwood two specimens of the soil of the cavern of Kühloch taken at different depths, in order that I might analyse them. This cavern contains a great number of fossil bones, belonging to carnivo-

rous and herbivorous animals, which Mr. Buckland conceives were not transported by water into the situation in which they are now seen, but that the Kühloch cavern was the haunt of carnivorous animals which died there; and their fossil bones are now found in a state of greater or less decay according to the degree of exposure to the atmosphere that they have undergone.

The letter A denotes a specimen of the soil taken at the depth of two feet, B one at six feet below the surface.

Both the specimens are, in great measure, in a pulverulent state, containing small masses which easily crumble to pieces; their colour is orange brown, pretty much like that of some bog iron ores (*mines de fer hydratées limoneuses*). The colour resides principally in the finest particles, as is evident if we agitate the specimens in water, and decant the fluid before it has become clear; the pulverulent particles remain suspended, while a granular sandy matter subsides of a yellowish gray colour; when a depsosit has formed from the muddy water which had been decanted off, it is found to have a fine orange yellow colour. The specimen A contains a smaller proportion of pulverulent particles than B, and is also less coloured.

Previous trials having shown that the matter soluble in water was in part alterable by the action of heat, like organic substances, I submitted both specimens to two series of experiments, to determine first the nature of the substances indestructible by heat, and secondly that of the matter destructible by that agent.

The results of the first series of experiments were as follows:

A.	Grammes.
Water, and matter volatile at 250°	0·185
Matter volatilized by combustion and a red heat	0·165
Sandy residuum: Silica	0·159
Sandy residuum: Alumina	0·026
Sandy residuum: Peroxide of iron	0·013
Sandy residuum: Lime	0·005
Phosphates of lime, magnesia, iron?	0·505
Carbonate of lime	0·624
Carbonate of magnesia	0·268
Sulphate of lime	0·024
	1·974
Loss	0·026
	2·000

B.	Grammes.
Water, and matter volatile at 250°	0·215
Matter volatilized by combustion and a red heat	0·200
Sandy residuum: Silica	0·185
Sandy residuum: Alumina	0·040
Sandy residuum: Peroxide of iron	0·013
Sandy residuum: Lime	0·002
Phosphates of lime, magnesia, iron?, manganese?	0·635
Carbonate of lime	0·459
Carbonate of magnesia	0·124
Sulphate of lime	0·027
Silica	0·020
	1·920
Loss	80
	2·000

I. The

The loss must be rather greater in reality than is indicated in the preceding tables, because the carbonates of lime and magnesia must have lost a portion of their carbonic acid by calcination; but the effervescence produced during the solution of the calcined matters in nitric acid (3) and (3), proves that the whole of the carbonic acid had not been volatilized by the calcination. I should add, that I looked in vain for fluoric acid in the soil of the cavern of Kühloch.

M. Chevreul then details the results of his experiments on the nature of the matter alterable by heat; and draws the subjoined conclusions from his entire examination.

I. The organic matter of the soil of the cave of Kühloch, destructible by fire, is formed of

1st, A fatty acid, which in my examinations presented the properties of stearic or margaric acid. 2dly, A fatty matter which was not acid. 3dly, An organic acid soluble in water. 4thly, A yellow colouring principle. 5thly, A brown azotized matter.

A portion of the yellow colouring principle and of the azotized matter is certainly combined with alumina and peroxide of iron. It is probable that another portion of the organic matters is united with the subphosphates and the subcarbonates of lime and magnesia; it is also probable that in this latter part, there is proportionally more azotized matter than in the former.

There is more organic and pulverulent matter in the specimen taken from a depth of six feet, than in that from a depth of only two feet.

II. There is in the soil some chloride of potassium and ammoniaco-sulphate of potash. Consequently the chloride of potassium and the sulphate of potash arising from the decomposition of the ammoniaco-sulphate of potash by heat, which could not be collected in the process adopted in the analysis of the incinerated soil, must augment the loss occurring in the analysis.

III. The proportions of sulphate of lime indicated in the ashes of the soil, are not so great as those which really exist in it; because, during the calcination, a portion of sulphuric acid is decomposed.

IV. It is probable that a portion of phosphate of magnesia is combined with phosphate of ammonia.

MINERAL SPRINGS OF THE CAUCASUS.

The following analyses of the springs of Alexander, near Kislavódskii, in the Caucasus, are given by Dr. Lyall in his interesting Travels in Russia, the Caucasus, &c. lately published; from a work respecting them by Dr. Haas, of Moscow.

In ten Pounds of twelve Ounces.	Great Warm Spring. (*Goriatchi Vodi.*)	Mary's Warm Spring.	Elizabeth's Spring.	Catherine's Spring.	Constantine's Spring.
Muriate of soda...........	91·24	94·59	76·38	67·10	26·57
Sulphate of soda..........	68·66	68·59	57·28	34·37	32·64
Carbonate of soda........	1·92	2·92	1·30	50·35	18·53
Carbonate of lime........	64·25	57·50	47·25	11·15	57·54
Carbonate of magnesia ...	16·00	18·00	12·00	4·00	—
Alumina, with a little magnesia, and a trace of iron	0·50	1·00	—	—	—
Oxide of iron, with a little alumina and magnesia.	—	—	0·50	0·25	10·52
Silica	6·12	10·00	7·00	2·12	4·18
Fetid sulphurous resin*	0·70	0·65	0·35	0·29	—
Extractif..................	?	?	?	0·60	?
	249·39	253·25	202·14	170·58	139·98

DESCRIPTION OF MEXICAN COAL.

The mineral treasures now laid open to the skill and enterprise of British adventurers in South America are daily exciting an increased interest throughout the kingdom: and as connected with the powerful machinery that will be employed in these undertakings, the subject of fuel becomes one of the greatest importance. The woods and forests, which once clothed the sides of the Cordilleras in the vicinity of the principal mines, have been, for many years, gradually diminishing, and in many places have totally disappeared; while the Mexican proprietors, with singular negligence, have forgotten to form new plantations to supply that enormous quantity of fuel necessary for the mines.

The existence of coal on the mining provinces of Mexico has hitherto been very doubtful. Humboldt, indeed, mentions its having been found in New Mexico; and that the formations of basalt and amygdaloid on the estates of the Count de Regla might lead to the belief that this substance also would probably be discovered; a supposition likewise entertained by Mr. John Taylor, whose practical and scientific knowledge of mining is well known. These opinions are now completely verified; as among the mineral productions brought by Mr. Bullock from Mexico are specimens of a coal analogous to jet, which he procured while residing in the vicinity of Real del Monte. A small piece of this substance, weighing sixteen grains, has been analysed by Dr. Trail; and the result of his experiments, con-

* Résine sulphureuse fétide. Stinkendes Schwefelharz of Westrumb.

tained

tained in a letter to Mr. Swainson, is expressed in the following words:—

"This specimen is more analogous to jet than to our Wigan Cannel coal. Its colour is deep brownish black; its lustre resinous; its cross fracture conchoidal; its longitudinal fracture has a slightly fibrous appearance, as if it had originally been wood. Its hardness is about that of Cannel coal, as is its frangibility; but its lustre is higher. The mean of three careful experiments gave its specific gravity = 1·2248. It becomes considerably electric by friction; in this character it is analogous to jet, and differs from Cannel coal, which scarcely shows any symptoms of electricity by friction, though I have observed that some pieces of the latter slightly moved an insulated cat's hair, which is a very delicate electroscope. Kirwan considers the difference between jet and Cannel in their electric energies, as a diagnostic mark.

"It burns with a lively flame, and gives out much liquid bituminous matter, or coal tar, so as to cake or become semiliquid in the fire. It does not decrepitate when heated, like Cannel. When heated before the blowpipe, in a glass tube, its volatile parts are separated; and it leaves behind about 50 per cent of a coke which is capable of exciting a pretty strong heat. The volatile portion affords a very pure coal gas. Six grains of it burnt in a platina crucible left behind 0·2 grains of greyish white ash, which is equivalent to $2\frac{1}{3}$ per cent of incombustible matter in it.

"The smallness of the specimen rendered it impossible to ascertain the relative quantities of carbon, hydrogen, and nitrogen, which similar substances contain; but this sort of analysis is rather an object of curiosity than utility."—*Quarterly Journal.*

REMARKABLE CASES OF THE FORMATION OF AMMONIA.

We extract the following remarkable cases of the formation of ammonia from a paper on the subject by Mr. Faraday in the Quarterly Journal of Science, No. XXXVII.

Having occasion, some time since, to examine an organic substance with reference to any nitrogen it might contain, I was struck with the difference in the results obtained, when heated alone in a tube, or when heated with hydrate of potassa: in the former case no ammonia was produced; in the latter, abundance. Supposing that the potash acted, by inducing the combination of the nitrogen in the substance with hydrogen, more readily than when no potash was present, and would, therefore, be useful as a delicate test of the presence of nitrogen in bodies, I was induced to examine its accuracy by heating it with substances containing no nitrogen,

as

as lignine, sugar, &c.; and was surprised to find that ammonia was still a result of the experiment. This led to trials with different vegetable substances, such as the proximate principles, acids, salts, &c., all of which yielded ammonia in greater or smaller quantity; and, ultimately, it was found that even several metals when treated in the same way gave similar results; a circumstance which appeared considerably to simplify the experiment.

The experiment may be made in its simplest form in the following manner: Put a small piece of clean zinc foil into a glass tube closed at one end, and about one-fourth of an inch in diameter; drop a piece of potash into the tube over the zinc; introduce a slip of turmeric paper slightly moistened at the extremity with pure water, retaining it in the tube in such a position that the wetted portion may be about two inches from the potash; then holding the tube in an inclined position, apply the flame of a spirit lamp, so as to melt the potash, that it may run down upon the zinc, and heat the two whilst in contact, taking care not to cause such ebullition as to drive up the potash: in a second or two, the turmeric paper will be reddened at the moistened extremity, provided that part of the tube has not been heated. On removing the turmeric paper and laying the reddened portion upon the hot part of the tube, the original yellow tint will be restored: from which it may be concluded that ammonia has been formed; a result confirmed by other modes of examination to be hereafter mentioned.

The first source of nitrogen which suggested itself was the atmosphere: the experiment was therefore repeated, very carefully, in hydrogen gas, but the same results were obtained.

The next opinion entertained was, that the potash might have been touched accidentally by animal or other substances, which had adhered to it in sufficient quantity to produce the ammonia: the alkali was therefore heated red hot, as a preparatory step, and afterwards allowed to touch nothing but clean glass or metals; but still the same effects were produced. The zinc used was selected from a compact piece of foil, was well rubbed with tow dipped in alkali, washed in alkaline solution, afterwards boiled repeatedly in distilled water, and dried, not by wiping, but in a hot atmosphere; and yet the same products were obtained.

All these precautions, with regard to impurity from fingering, were found to be essentially requisite, in consequence of the delicacy of the means afforded by heat and turmeric paper for testing the presence of ammonia, or rather, of matter containing its elements. As a proof of this, it may be mentioned, that some sea sand was heated red hot for half an hour in a

crucible

crucible, and then poured out on to a copper-plate, and left to cool: when cold, a portion of it (about 12 grains) was put into a clean glass tube; another equal portion was put into the palm of the hand, and looked at for a few moments, being moved about by a finger, and then introduced by platina foil into another tube, care being taken to transfer no animal substance but what had adhered to the grains of sand: the first tube when heated yielded no signs of ammonia to turmeric paper, the second a very decided portion.

As a precaution, with regard to adhering dirt, the tubes used in precise experiments were not cleaned with a cloth, or tow, but were made from new tube, the tube being previously heated red hot, and air then drawn through it; and no zinc or potash was used in these experiments, except such as had been previously tried by having portions heated in a tube, to ascertain whether when alone they gave ammonia.

It was then thought probable that the alkali might contain a minute quantity of some nitrous compound, or of a cyanide, introduced during its preparation. A carbonate of potash was therefore prepared from pure tartar, rendered caustic by lime calcined immediately preceding its use, the caustic solution separated by decantation from the carbonate of lime, not allowed to touch a filter or any thing else animal or vegetable, and boiled down in clean flasks; but the potash thus obtained, though it yielded no appearance of ammonia when heated alone, always gave it when heated with zinc.

The water used in these experiments was distilled, and in cases where it was thought necessary was distilled a second, and even a third time. The experiments of Sir Humphry Davy * show how tenaciously small portions of nitrogen are held by water, and that, in certain circumstances, the nitrogen may produce ammonia. I am not satisfied that I have been able to avoid this source of error.

At last, to avoid every possible source of impurity in the potash, a portion of that alkali was prepared from potassium; and as the experiment made with it includes all the precautions taken to exclude nitrogen, I will describe it rather minutely, as illustrative of the way in which the other numerous experiments were made. A piece of new glass tube, about half an inch in diameter, was first wiped clean, and then heated red hot, a current of air passing at the same time through it; about six inches in length was drawn off at the blow-pipe lamp, and sealed at one extremity. Some distilled water was put into a new glass retort, and heated by a lamp; when about

* Phil. Trans. 1807, p. 11.

one

one half had distilled over, the beak of the retort was introduced into the tube before mentioned, and a small portion of water (about fifty grains) condensed into it. A solid compact piece of potassium was then chosen out, and having been wiped with a linen cloth, was laid on a clean glass plate, the exterior to a considerable depth removed by a sharp lancet, and portions taken from the interior by metallic forceps, and dropped successively into the tube containing the water before mentioned. Of course the water was decomposed, and the tube filled with hydrogen; and when a sufficient quantity of solution of potash had been thus formed, the tube was heated in a lamp, and drawn out to a capillary opening, about two inches from the closed extremity. The tube now formed almost a close vessel; and being heated, as the water became vapour, it passed off at the minute aperture, and ultimately a portion of pure fuzed hydrate of potassa remained in the bottom of the tube. The aperture of the tube was now closed, and the whole set aside to cool.

A piece of new glass tube was selected about 0·3 of an inch in diameter; it was heated to dull redness, and air passed through it: about ten inches of it was then cut off, and being softened near to one end by heat, it was drawn out at that part until of small diameter: that part was then fixed into a cap, by which it could afterwards be attached to a receiver containing hydrogen. The tube containing the potassium potash being now broken in an agate mortar, a piece or two of the potash was introduced by metallic forceps into the tube at the open end, so as to pass on to the contracted part; a roll of zinc foil, about one grain in weight, cleaned with all the precautions already described, was afterwards introduced, and then more of the potash. The tube was then bent near the middle to a right angle; a slip of turmeric paper introduced, so as just to pass the bend; and thus prepared, it was ready to be filled with hydrogen.

The precautions taken with regard to the purity of the hydrogen were as follows: a quantity of water had been put into a close copper boiler, and boiled for some hours, after which it had been left all night in the boiler to cool. A pneumatic trough was filled with this water just before it was required for use. The hydrogen was prepared from clean zinc, which being put into a gas bottle, the latter was filled entirely with the boiled water, and then sulphuric acid being poured in through the water, the gas was collected, the excess of liquid being allowed to boil over. The hydrogen was received in the usual manner into jars filled with the water of the trough, the transferring jar, when filled, being entirely immersed

immersed in the water, so as to exclude the air from every part, even of the stop-cock. The first jar of gas was thrown away, and only the latter portions used.

The gas being ready, the experimental tube was attached to the transferring jar by a connecting piece, so that the part of it containing the zinc and potash was horizontal, whilst the other portion descended directly downwards. A cup of clean mercury, the metal being about an inch in depth, was then held under the open end of the tube, and by lowering the jar containing the hydrogen in the water of the pneumatic trough, so as to give sufficient pressure, and opening the stop-cock, the hydrogen in the jar was made to pass through the tube, and sweep all the common air before it. When from 100 to 150 cubic inches, or from 200 to 300 times the contents of the tube, had passed through, the cup of mercury was raised as high as it could be, so as to prevent the passage of any more gas, the pressure from the jar in the water-trough was partly removed, and the stop-cock closed; then, by lowering the cup of mercury a little, the surface of the metal in it was made lower than that within the tube; and in this state of things the flame of a spirit lamp applied to the contracted part of the tube sealed it hermetically, without the introduction of any air, and separated the apparatus from the jar on the water-trough.

In this way every precaution was taken that I could devise for the exclusion of nitrogen; yet, when a lamp was applied to the potash and zinc, the alkali no sooner melted down and mingled with the metal, than ammonia was developed; which rendered the turmeric paper brown, the original yellow reappearing by the application of heat to the part.

With regard to the evidence of the nature of the substance produced, it was concluded to be ammonia in the experiments made in hydrogen, from its changing the colour of turmeric paper to reddish brown; from the disappearance of the reddish brown tint and re-production of yellow colour by heat; from its solubility in water, as evinced by the greater depth of colour on moist turmeric paper than on dry; from its odour; and from its yielding white fumes with the vapour of muriatic acid. When formed in open tubes, its nature was still further tested by its neutralizing acids and restoring the blue colour of reddened litmus paper; by its rendering a minute drop of sulphate of copper on a slip of white paper deep blue; and also, at the suggestion of Dr. Paris, by introducing into it a slip of paper moistened in a mixed solution of nitrate of silver and arsenious acid, the yellow tint of arsenite of silver being immediately produced.

Potash is not the only substance which produces this effect with the metals and vegetable substances. Soda produces it; so also does lime, and baryta,—the latter not being so effective as the former, or producing the phænomena so generally. The common metallic oxides, as those of manganese, copper, tin, lead, &c., do not act in this manner.

Water or its elements appear to be necessary to the experiment. Potash or soda in the state of hydrates generally contain the water necessary. Potash, dried as much as could be by heat, produced little or no ammonia with zinc; but redissolved in pure water and evaporated, more water being left in it than before, it was found to produce it as usual. Pure caustic lime, with very dry linen, produced scarcely a trace of ammonia, whilst the same portion of linen with hydrate of lime yielded it readily.

The metals when with the potash appear to act by, or according to, their power of absorbing oxygen. Potassium, iron, zinc, tin, lead, and arsenic evolve much ammonia, whilst spongy platina, silver, gold, &c., produce no effect of the kind. A small portion of fine clean iron wire dropped into potash melted at the bottom of a tube, caused the evolution of some ammonia, but it soon ceased, and the wire blackened upon its surface: the introduction of a second portion of clean wire caused a second evolution of ammonia. Clean copper wire, in fused potash, caused a very slight evolution of ammonia, and became tarnished.

The following, among other vegetable substances supposed to contain no nitrogen, have been tried with potash in tubes open to the air: lignine, prepared by boiling linen in weak solution of potash, then in water, afterwards in weak acid, and finally in water again; oxalate of potassa, oxalate of lime, tartrate of lead, acetate of lime, asphaltum, gave very striking quantities to turmeric and litmus paper; acetate of potash, acetate of lead, tartrate of potash, benzoate of potash, oxalate of lead, sugar, wax, olive-oil, naphthaline, produced ammonia, but in smaller quantity; resin appeared to yield none, nor when potash was heated in the vapour of alcohol or ether, or in olefiant gas, could any ammonia be detected.

It may be remarked, that much appeared to depend upon the quantity of potash used: sugar, for instance, which with a little potash would with difficulty yield traces of ammonia, does so very readily when the quantity of potash is doubled or trebled; and linen, which with potash gives ammonia very readily, yields it the more readily, and in greater quantity, as the proportion of potash is increased.

PROFESSOR

PROFESSOR GMELIN ON A NEW FORMATION OF ANHYDROUS SULPHURIC ACID.

It has been an opinion hitherto received, that anhydrous sulphuric acid can be obtained in no other way than by decomposing in a distillatory apparatus such sulphates as, when heated, give off their acid—such as calcinated iron-vitriol. It is generally known that the fuming oil of vitriol from Nordhausen is procured in this way. I have found that the *not fuming* (so called in English) oil of vitriol yields at a certain period of the distillation fuming acid. I heated in a retort, connected with a receiver, 6 pounds 14½ ounces English oil of vitriol, of a specific gravity, = 1·8435 at + 10½° R., which was not the least fuming. The acid never came to boiling; the temperature of the air was 0° R., four ounces having distilled, having a strong smell of sulphurous acid; the receiver was emptied, cleansed, and applied anew. When eight ounces of an acid, which was quite destitute of smell, had distilled over, the receiver, which had hitherto been perfectly transparent, was suddenly filled with vapours. It was removed, and another dry receiver applied, which was now surrounded with powdered ice. There was condensed an acid partly not transparent, partly transparent and crystalline; a good deal of the solid acid was found in the neck of the retort. This solid acid was exceedingly fuming, like that produced from the fuming oil of vitriol; it remained solid at + 12° R., and had no smell of sulphurous acid. When brought in contact with a certain quantity of sulphur, in a close air-tight glass vessel, a green compound, having the colour of muriate of chrome, was formed, and a little sulphurous acid was disengaged. This green mass being brought in contact with water, a very great heat was evolved, sulphurous acid formed, and sulphur dissolved. When the solid acid was brought in contact with water, diluted acid was formed, but no sulphurous acid. This diluted acid being saturated by potash, and evaporated to crystallization, no nitre was formed, nor were nitrous vapours produced by heating the dry mass with concentrated sulphuric acid. The specific gravity of the acid left in the retort, *which was now sensibly fuming*, was found = 1·8503 at + 13° R.; the specific gravity of the acid which distilled over, = 1·4309 at + 11½° R.* This experiment being repeated with the same acid, the same result was obtained. But it may happen that the moment at which the fuming acid is formed is overlooked: in the experiments just now mentioned it was

* The specific gravities were determined by means of a small bottle, provided with a plate ground upon its neck.

 not

not formed; but in the first half of the third day (during the two first days, from seven o'clock in the morning till nine o'clock in the evening), fire had been kept in the furnace, and its formation could not longer be perceived than during about half an hour *. These experiments leave, I think, no doubt that the solid acid really was anhydrous sulphuric acid. Its formation may thus be explained,— that, in a certain concentration of the aqueous sulphuric acid, part of the acid yields its water to another part of the acid, and is volatilized; whereby, on one side, by the great volatility of the anhydrous acid, on the other side by the great fixity of the acid containing water, this kind of decomposition seems to be induced.—*Brewster's Journ.*

MR. FARADAY ON THE COMPOSITION OF CRYSTALS OF SULPHATE OF SODA.

It is known that when a hot strong solution of sulphate of soda is put into a vessel and closed up, it may be reduced to common temperatures without crystallizing, although, if the vessel be opened, abundance of crystals will immediately form. It has also frequently been observed that, in some circumstances, crystals would form in the solution during cooling, even though the vessel had not been opened or agitated. These crystals, when observed in the solution, are very transparent and of a large size; they are quadrangular prisms, with diedral summits. Upon opening the vessel, the surrounding solution crystallizes rapidly, enveloping the first formed set of crystals with others, which, however, are very readily distinguished from them in consequence of their immediately assuming a white opaque appearance. Upon taking out the crystals, those first formed are found to be much harder than the usual crystals of sulphate of soda; and when broken, it is found that the opacity is not merely superficial, but that it penetrates them to a considerable depth, and even at times throughout.

These harder and peculiar crystals are readily obtained by closing up a solution of sulphate of soda, saturated at 180°, in a Florence flask, boiling the solution in the flask so as to expel the air before closing it. Upon standing 24 hours, fine groups of crystals are formed. When the flask is opened, the solution deposits fresh crystals; but on breaking the flask, the latter may be scraped off by a knife, in consequence of the superior hardness of the first set.

* I quote these circumstances, that it may appear how slowly the distillation proceeded. Probably no fuming acid will be formed when the fluid in the retort is brought to boiling.

The

The hard crystals when separated are found to be efflorescent, like those of the usual kind; and they ultimately give off all their water, leaving only dry sulphate of soda. When a given weight was heated in a platina crucible, one half their weight passed off as water, the rest being dry salt. They, consequently, contain eight proportionals of water, or 72 sulphate of soda, and $8 \times 9 = 72$ water. The usual crystals of sulphate of soda contain 10 proportionals of water.

When crystallized sulphate of soda is heated in a flask, a part of it dissolves in the water present, whilst the rest is thrown down in an anhydrous state. The solution at 180° appears to contain one proportional of salt 72, and 18 proportionals of water 162; from which, if correct, it would result, that when the crystals are heated to 180°, $\frac{5}{9}$ of the salt take all the water, whilst $\frac{4}{9}$ separate in the dry state.—*Quarterly Journal.*

INFLUENCE OF COPPER, ETC., ON MAGNETIC NEEDLES.

M. Arago communicated to the Academy of Sciences his experinents relative to the oscillations of a magnetic needle surrounded by different substances. He had ascertained that the copper rings with which dipping-needles are generally surrounded exerted on the needles a very singular action, the effect of which was rapidly to diminish the amplitude of the oscillations without sensibly altering their duration. Thus when a horizontal needle suspended in a ring of wood by a thread without tension was moved 45° from its natural position, and left to itself, it made 145 oscillations before the amplitude was reduced to 10°. In a ring of copper, the amplitude diminished so rapidly that the same needle, removed 45° from its natural position, only oscillated 33 times before the arc was reduced to 10°. In another ring of copper, of less weight, the number of oscillations between the arcs of 45° and 10° were 66. The time of the oscillations appeared to be the same in all the rings.

In the ring of wood	145	oscillations from	45° to	10°
——————— copper	33	. . . .	45	10
In a lighter copper ring	66	. . . .	45	10.

Rév. Ency.

MEDICINAL LEECHES.

A report has lately been laid before the French Academy of Sciences, by MM. Duméril and Latreille, on a memoir by MM. Pelletier and Huzard, jun., containing researches upon leeches.

"The authors of this memoir had been commissioned to obtain information for the civil authorities relative to the means of putting an end to the complaints which are often made

made to them respecting the bad quality of the leeches employed in medicine. The two chief points which they proposed to examine, are, 1st, To ascertain the causes which in certain cases render the little wounds made by these animals difficult to cure. 2dly, To examine the circumstances under which certain leeches do not penetrate the skin to which they are applied. On the first point authors agree with physicians in acknowledging that the inconveniences ascribed to leeches ought most frequently to be attributed either to the temperament of the patient, or to the nature of the malady, or to the means employed to detach them from the wound, or to the foreign substances employed for staunching the blood and closing the wound. With regard to the second point, MM. Huzard and Pelletier have found that there are offered for sale species of leeches that at first sight entirely resemble medicinal leeches, but which differ from them completely; 1st, in their want of the serrated instrument proper to make the incisions in the skin, from which issues the blood that the animal sucks; 2dly, in the conformation of their stomach and intestinal canal. The experiments of the authors have proved to them that the spurious leeches cannot be employed in medicine, because they do not bite. M. Dutrochet has already described, in 1817, the *Annelide* offered as a new species by MM. Huzard and Pelletier; but there crept into his work some errors relating to the manners, the habits, and the organization of this animal, which for a year has been the particular object of the authors."—*Rév. Ency.* Feb. 1825.

LIST OF NEW PATENTS.

To Robert Hicks, of Conduit-street, surgeon, for an improved bath.—Dated 22d March 1825.—6 months to enrol specification.

To Francis Ronalds, of Croydon, Surrey, esq. for a new tracing apparatus to facilitate the drawing from nature.—23d March.—2 months.

To Richard Wilty, of Kingston-upon-Hull, civil engineer, for an improvement in the method of lighting by gas, by reducing the expense thereof.—25th March.—6 months.

To John Martin Hanchelt, of Crescent-place, Blackfriars, London, and Joseph Delvalle, of Whitecross-street, Middlesex, esqrs. for an improvement, communicated from abroad, in looms for making cloths, silks, and different kinds of woollen stuffs, of various breadths.—25th March.—6 months.

To Joseph Manton, of Hanover-square, gun-maker, for an improvement in shot.—25th March.—6 months.

To John Gotlieb Ulrich, of Bucklersbury, London, chronometer-maker, for improvements on chronometers.—25th March.—6 months.

To Aaron Jennins and John Belteridge, both of Birmingham, japanners, for improvements in the method of preparing and working pearl-shell into various forms and devices, for the purpose of applying it to ornamental uses in the manufacture of japan ware and other articles.—29th March.—6 months.

To

To Richard Roberts, of Manchester, civil engineer, for improvements in the mule, billy, jenny, stretching frame, or other machines used in spinning cotton, wool, or other fibrous substances, and in which either the spindles recede from and approach the rollers or other deliverers of the said fibrous substances, or in which such rollers or deliverers recede from and approach the spindles.—29th March.—6 months.

To James Haumer Baker, of Antigua, (now residing in St. Martin's-lane,) for improvements in dyeing and calico-printing by the use of certain vegetable materials.—29th March.—6 months.

To Maurice de Jongh, of Warrington, cotton-spinner, for improvements in spinning machines, and mules, jennies, slubbers, &c.—29th March.—6 months.

To Edward Sheppard, of Uley, Gloucestershire, clothier, and Alfred Flint, of the same place, engineer, for improvements in machinery for raising the wool or pile on woollen or other cloths by points, also applicable to brushing, smoothing, and dressing cloths.—29th March.—2 months.

To Thomas Parkin, of Bache's-row, City Road, Middlesex, merchant, for a mode of paving parts of public roads, whereby the draft of waggons, carts, coaches and other carriages is facilitated.—29th March.—6 months.

To Rudolphe Cabanel, of Melina-place, Westminster Road, Lambeth, engineer, for improvements on engines or machinery for raising water, part of which machinery is applicable to other useful purposes.—30th March.—6 months.

To John Heathcoat, of Tiverton, Devon, lace-manufacturer, for improved methods of figuring or ornamenting various goods manufactured from silk, cotton, flax, &c.—31st March.—6 months.

To Jacob Jedder Fisher, of Ealing, Middlesex, esq. for a new application of rail-ways, and the machinery to be employed thereon.—2d April.—6 months.

To Simeon Broadmeadow, of Abergavenny, Monmouthshire, civil engineer, for his apparatus for exhausting, condensing, or propelling air, smoke, gas, &c.—2d April.—6 months.

To William Turner, of Winslow, Cheshire, saddler, and William Mosedale, of Park-street, Grosvenor-square, coach-maker, for an improvement on collars for draft horses.—2d April.—2 months.

To Robert William Brandling, of Low Gosforth, near Newcastle-upon-Tyne, for improvements in rail-roads, and carriages to be employed thereon and elsewhere.—12th April.—6 months.

To William Shalders, of the city of Norwich, leather-cutter, for a gravitating expressing fountain for raising and conveying water or any other fluid, for any purpose.—12th April.—2 months.

To William Gilman, of Whitechapel Road, engineer, and James William Sowerby, of Birchin-lane, London, merchant, for improvements in generating steam, and on engines to be worked by steam or other elastic fluids. 13th April.—6 months.

To Thomas Sunderland, of Croom's Hill Cottage, Blackheath, Kent, esq. for a new combination of fuel.—20th April.—6 months.

To Charles Ogilvy, of Verulam-buildings, Gray's Inn, esq. for an improved apparatus for storing gas.—20th April.—6 months.

To John Broomfield, of Islington, near Birmingham, engineer, and Joseph Luckcock, of Edgbaston, near Birmingham, for improvements in the machinery for propelling vessels.—20th April.—6 months.

To Lemuel Wellman Wright, of Wellclose-square, Middlesex, engineer, for improvements in apparatus for washing or bleaching of linens, cottons, &c.—20th April.—6 months.

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport, Mr.* CARY *in London, and Mr.* VEALL *at Boston*

Days of Month, 1825.	Gosport, at half-past Eight o'Clock, A.M. Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Clouds. Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Height of Barometer, in Inches, &c. Lond. 1 P.M.	Bost. 8½ A.M.	Thermometer. London. 8 A.M.	Noon.	11 P.M.	Boston. 8½ A.M.	Rain. London.	Boston.	Weather. London.	Boston.	Wind.
Mar. 26	29·90	45	48·90	62	NE.	...	......	1	...	...	...	1	1	...	30·05	29·93	40	54	41	38·5	...	...	Fair	Fine	E.
☽ 27	29·98	44	......	58	NE.	...	......	1	1	1	...	...	...	...	30·08	29·90	40	51	42	43	...	...	Fair	Cloudy	Calm
28	30·00	44	......	60	NE.	0·45	......	1	1	1	1	...	...	1	30·05	29·85	42	51	43	46	...	...	Cloudy	Fine	Calm
29	29·90	44	......	67	NE.	...	......	...	...	1	...	...	1	1	29·98	29·80	38	45	41	41	...	...	Cloudy	Fine	S.
30	29·96	44	......	68	NE.	...	......	...	...	1	...	...	1	...	30 05	29·85	42	44	40	42·5	...	...	Cloudy	Cloudy	E.
31	30·15	45	48·90	63	E.	·40	......	1	...	...	...	1	...	...	30·33	30·10	41	50	40	43·5	...	...	Fair	Fine	NE.
April 1	30·34	43	......	58	NE.	...	......	1	1	1	...	...	...	...	30·47	30·33	38	49	36	41·5	0·00	...	Fair	Fine	NE.
2	30·42	42	......	55	NE.	...	......	1	1	1	...	...	...	...	30·44	30·30	35	56	40	41·5	...	...	Fair	Fine	NW.
○ 3	30·34	46	......	64	S.	·40	......	1	...	1	1	...	...	...	30·34	30·17	40	61	45	47	...	...	Fair	Fine	NW.
4	30·20	48	......	55	N.	...	......	1	1	1	..	...	...	...	30 27	30·02	44	63	44	48	...	...	Fair	Fine	NW.
5	30·25	48	......	61	SE.	...	......	1	...	1	...	...	...	...	30·30	30·05	40	60	40	47·5	...	...	Fair	Fine	Calm
6	30·26	47	......	62	E.	·50	......	1	...	...	...	...	...	...	30·37	30·15	40	55	42	43	...	...	Fair	Fine	E.
7	30·28	50	48·90	56	NE.	·25	......	1	...	...	...	...	...	...	30·40	30·18	41	55	42	48	...	...	Fair	Fine	E.
8	30·31	50	......	57	NE.	·40	......	...	...	...	...	...	...	...	30·41	30·10	44	56	42	49	0·00	...	Fair	Fine	E.
9	30·20	50	......	58	N.	·25	......	1	...	1	1	...	...	...	30·24	30·00	39	57	48	50	...	...	Fair	Fine	W.
☾ 10	30·23	53	......	59	SW.	·20	......	1	...	1	1	...	...	...	30·31	30·00	48	60	50	52	...	...	Hazy	Fine	E.
11	30·14	50	......	56	W.	·15	......	1	1	1	...	1	...	...	30·12	29·80	50	66	51	51·5	...	...	Fair	Fine	W.
12	30·06	52	......	58	NW.	·20	......	1	1	1	...	1	...	...	30·10	29·70	50	56	50	51·5	...	...	Fair	Stormy	W.
13	30·05	51	......	60	W.	...	......	1	...	1	...	...	1	...	30·12	29·75	50	50	45	41·5	...	...	Cloudy	Rain	NW.
14	30·14	56	49·00	61	SW.	...	......	1	1	1	...	1	...	...	30·12	29·74	46	64	50	51	...	·03	Fair	Cloudy	S.
15	30·19	53	......	61	W.	·40	......	1	...	1	...	1	...	...	30 20	29·80	50	65	51	53 5	0·00	...	Fair	Fine	N.
16	30·16	52	......	63	W.	...	......	1	...	1	...	1	...	...	30·12	29 75	51	63	50	55	...	...	Fair	Cloudy	W.
17	30·18	53	......	56	N.	...	......	1	1	1	...	1	...	...	30·26	30·00	46	53	40	44	...	...	Fair	Cloudy	NW.
18	30·19	45	......	50	NE.	·60	......	1	...	1	...	1	...	...	30·20	29·98	40	51	39	43·5	...	...	Fair	Fine	N.
19	30·21	45	......	50	NE.	...	......	...	...	...	...	1	...	...	30·25	30·02	39	49	40	41·5	...	...	Fair	Fine	NW.
20	30·15	53	49·00	54	SW.	...	0·005	...	...	...	...	...	1	1	30·15	29·88	45	56	50	44	...	...	Cloudy	Cloudy	NW.
21	30·00	54	......	63	SW.	·38	·350	1	1	1	...	...	1	1	29·96	29·63	50	61	50	53	...	...	Fair	Fine	W.
22	29·60	53	......	76	SW.	...	·310	...	...	1	...	...	...	1	29·65	29·33	50	58	51	52·5	...	...	Showery	Fine, rain at night	W.
23	29·43	55	......	68	SW.	...	·170	1	...	1	...	1	...	1	29·51	29·22	51	64	50	48	...	·86	Fair	Rain	NE.
24	29·36	57	......	69	E.	...	·420	1	1	1	...	1	1	1	29·44	29·30	50	56	47	45·5	...	·42	Showery	Rain	Calm
25	29·63	58	49·05	66	SE.	·30	......	1	1	1	...	1	1	...	29·74	29·53	50	60	49	46	0·80	·24	Showery	Cloudy	E.
Aver.	30·071	49·36	48·96	60·4		4·88	1·255	25	13	24	4	13	8	7	30·13	29·87	44	56	45	46·5	0·8[illegible]	1·55			

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LIII. *On the Binomial Theorem, and the Application of some Properties of* $\Delta^m . 0^n$ *to General Differentiation and Integration.* *By* JOHN HERAPATH, *Esq.*

FROM the time I first became acquainted with the fluxional discoveries of Newton, it forcibly struck me that his calculus was only a particular branch of a much more general one. I felt, therefore, surprised that mathematicians had suffered upwards of a century to pass without any attempt, I might almost say without even a hint, towards its generalization. They have given us methods by which we can generally, by successive operations, obtain the value of any order denoted by an integer of the differentials or integrals of a function; but to extract, if one may so call it, any differential root, or even to assign the value of $d^r . x^n$ when r is only a fraction, to say nothing of its being an irrational or imaginary number, seems to have been looked on as an impossibility. Lacroix, I have heard, has said something of fractional differentials in his great work on this calculus; but he considers them, it seems, as quantities of a different kind from integer differentials. M. Brisson too in the *Journal Polytechnique*, 14 cahier, page 199, has given a series for $\frac{d^n u_x}{dx^n}$ whatever be the value of n; but the series is of a most troublesome description, and by proceeding according to the integer differentials of $\frac{u_x}{x}$ is, in my opinion, nearly if not quite useless. The development of $d^n u_x$ may, if that only be sought, be obtained by a much simpler and neater method.

Mr. Babbage has likewise in one of his functional papers incidentally alluded to fractional differentials in mentioning M. Lacroix's opinion; but he has not that I remember said any thing which might lead to their calculus.

I should also mention that Mr. Herschel, in a letter dated

Now, if $n=r+v$, and n be as before an integer, r or v may be any number rational, irrational, or imaginary; and since the sum n of these numbers is an indeterminate positive integer, they will in point of value be independent. But

$$q_{r+v}=q_n=n.\frac{n-1}{2}.\frac{n-2}{3}\ldots=(r+v)\frac{r+v-1}{2}.\frac{r+v-2}{3}\ldots$$

or

$$\frac{n^{q-1}+an^{q-2}+an^{q-3}\ldots}{1.2\ldots(q-1)}=\frac{(r+v)^{q-1}+a(r+v)^{q-2}+a_{\prime}(r+v)^{q-3}}{1.2\ldots(q-1)}$$

$$=q_r+2_v(q-1)_r\ldots q_v \qquad (F)$$

And because in the two right hand members of this (F), r and v are independent variables, these members when duly reduced must not contain any product of the powers of the variables; for if they did, the function of either variable would be affected by the changes of the other variable, which it should not. The middle member therefore of (F) must contain only the sums of the powers of r and v, and the right hand member only the two exterior terms. Consequently

$$q_r+q_v=\frac{(r^{q-1}+v^{q-1})+a(r^{q-2}+v^{q-2})+a_{\prime}(r^{q-3}+v^{q-3})\ldots}{1.2\ldots(q-1)}.$$

which again, on account of the independence of the functions, will evidently give

$$q_r=r\frac{(r-1)(r-2)\ldots}{1.2\ldots(q-1)} \text{ and } q_v=v\frac{(v-1)(v-2)\ldots}{1.2\ldots q-1)}$$

which completes the proof.

This conclusion we may easily verify by actual reduction. Thus $\quad 2_r+2_v=2_{r+v}=2_n=n=r+n$

which since 2_r and 2_v are independent, as well as r and n, give

$$2_r=r \text{ and } 2_v=v$$

and because

$$(r+v)\frac{r+v-1}{2}=3_r+2_v\cdot 2_r+3_v=3_r+vr+3_v$$

gives $\quad \dfrac{(r^2+v^2)-(r+v)}{1.2}=3_r+3_v,$

we have by reason of the independence of the functions

$$3_r=\frac{r.(r-1)}{1.2} \text{ and } 3_v=\frac{v.(v-1)}{1.2}$$

and so it may be shown of the rest.

We may here remark, that any coefficient of the binomial expansion, as q_r, may be represented by $\Delta^{2-q}r$, so that we have the binomial theorem under the new form

$(x+y)^r$.

$$(x+y)^r = x^r\left\{\Delta r + \frac{y}{x}\Delta^0 r + \frac{y^2}{x^2}\Delta^{-1} r + \frac{y^3}{x^3}\Delta^{-2} r \ldots\right\}$$

$$= x^r . \frac{\Delta^2}{\Delta - \frac{y}{x}} . r \qquad \text{(G)}$$

in which the abbreviated operation refers to r as the variable.

It should here also be remarked, that the binomial development terminates at the term immediately before that whose coefficient becomes $=0$; and if this does not happen, it goes on *ad infinitum*. We shall in more than one instance presently see the value of this apparently trifling observation.

On General Differentiation and Integration.

We will commence with the simple differential

$$d^r . x^n$$

whose numerical coefficient is to be determined, whatever be the values of r, n; for the value of the exponent is under all circumstances evidently $n-r$. Let us put n_r for the coefficient, and when r is a whole positive number the ordinary processes of differentiation give

$$n_r = n . \overline{n-1} . \overline{n-2} \ldots . \overline{n-(r-1)} \qquad (1)$$

or, $$n_r = n^r - an^{r-1} + a_{\prime} n^{r-2} - a_2 n^{r-3} \ldots a_{r-2} n \qquad (2)$$

terminating at the term immediately before that whose coefficient becomes $=0$. The same rule of termination also holds good in (1) when r is a whole number and $n=r$.

Now when r is a variable integer it is easy to prove that

$$n_r = n^r - n^{r-1}\Delta_{\prime}^{-1}.r^0 + n^{r-2}\Delta_{\prime}^{-2}.r^0 - n^{r-3}\Delta_{\prime}^{-3}.r^0 \ldots$$

$$= n^r \frac{\Delta_{\prime}}{\Delta_{\prime} + \frac{1}{n}} . r^0 \qquad (3)$$

the operations $\Delta_{\prime}^{-1}.r^0$ $\Delta_{\prime}^{-2}.r^0$, $\Delta_{\prime}^{-3}.r^0 \ldots$ being respectively equal to $\Delta^{-1} r$, $\Delta^{-1}.r\Delta^{-1} r$, $\Delta^{-1}.r\Delta^{-1}.r\Delta^{-1} r$ .. applied to unity or r^0. But since

$$\Delta_{\prime}^{-1}.r^0 = r\frac{r-1}{2},\ \Delta_{\prime}^{-2}.r^0 = \frac{r.r-1\ldots r-3}{2 \cdot 4} + \frac{r\ldots r-2}{3},$$

$$\Delta_{\prime}^{-3}.r^0 = \frac{r\ldots r-5}{2.4.6} + \frac{r\ldots r-4}{2.3} + \frac{r\ldots r-3}{4},$$

it is plain that the coefficient of any term of (3) is composed of factors similar to those of the development of the binomial, and will consequently admit a similar proof for non-integer values of r. Our (3) is therefore true for all values of r rational,

tional, irrational, or imaginary. We have hence a general expression for $d^r . x^n$ whether r and n be possible or imaginary numbers. Before, however, I make any observations on the properties of the numeral coefficient n_r I shall proceed to discover another expression much more elegant and commodious than our (3).

Thus
$$\Delta . x^v = (x+1)^v - x^v$$
$$\Delta^2 . x^v = (x+2)^v - 2(x+1)^v + x^v$$
$$\Delta^3 . x^v = (x+3)^v - 3(x+2)^v + 3(x+1)^v - x^v$$

and generally when n is any positive integer

$$\Delta^n . x^v = (x+n)^v - n(x+n-1)^v + n\tfrac{n-1}{2}(x+n-2)^v \ldots \quad (4)$$

continued to the term next preceding that whose coefficient becomes $=0$. But these coefficients are the same as the binomial, and have therefore the same proof for non-integer values of n. Hence (4) is true, whatever be the value of n, and we know it is true for all values of x and v; it is therefore true universally.

I shall show presently how to deduce from this finite expressions for negative integer values to n.

If we put $x=0$ we obtain the celebrated expression for $\Delta^n . 0^v$, namely

$$\Delta^n . 0^v = n^v - n(n-1)^v + n\tfrac{n-1}{2}(n-2)^v \ldots \quad (5)$$

true likewise for all values of n and v, whether possible or impossible.

This expression I sent between two and three years since to an able mathematician as true for imaginary values of n. It was of course given without demonstration, and hence he thought it merely a definition. The investigation, or rather the demonstration, I have now given it, will however, I presume, entitle it as much to the appellation of a theorem as the binomial expansion.

I have hitherto proceeded in an algebraico-demonstrative manner, which will easily lay open the process of the following investigations; I shall therefore be more concise in what I intend to add in this paper.

An easy management of (4) will bring it under the form

$$x^v t^x (t-1)^n + nt^{x+1}(t-1)^{n-1}\Delta . x^v + n\tfrac{n-1}{2}t^{x+2}(t-1)^{n-2}\Delta^2 . x^v \quad (6)$$

or, abbreviated,
$$t^x . \{t-1+t\Delta\}^n . x^v \quad (7)$$

which

which admits of an algebraic equality with $\Delta^n.x^v$ when $t=1$. From this expression we deduce

$$\Delta^n.0^n = n.n-1.n-2\ldots \qquad (8)$$

which terminates at the term immediately before that which becomes $=0$, or else proceeds on *ad infinitum* like the other expressions. Hence we easily get

$$\frac{d^r.x^n}{dx^r} = \frac{\Delta^n.0^n}{\Delta^{n-r}.0^{n-r}}.x^{n-r} \qquad (9)$$

whatever be the values of n and r.*

This is the commodious expression I before alluded to; and perhaps, independent of its importance and its containing the first attempt that has yet been made towards applying the properties of $\Delta^n.0^v$ to general differentiation, mathematicians will not think my anticipation of it, as an elegant theorem, unmerited.

We will now slightly glance at two or three of its consequences. Suppose r is any positive integer, n being any number, then

$$x^{n-r}.\frac{\Delta^n.0^n}{\Delta^{n-r}.0^{n-r}} = x^{n-r}.\frac{n.n-1\ldots n-r\ldots \text{to } 1 \text{ or } \infty}{n-r.\,n-r-1\ldots \text{to } 1 \text{ or } \infty}$$

$$= n\ldots n-r+1.x^{n-r} \qquad (10)$$

and if r be any negative integer, then

$$x^{n+r}.\frac{\Delta^n.0^n}{\Delta^{n+r}.0^{n+r}} = x^{n+r}.\frac{n.n-1\ldots \text{to } 1 \text{ or } \infty}{n+r\ldots n.n-1\ldots \text{to } 1 \text{ or } \infty} = \frac{x^{n+r}}{n+1\ldots n+r} \qquad (11)$$

which are known to be the rth differential and integral of x^n.

If n be a positive integer and $r>n$ it appears by (10) that the numeral coefficient $\quad n_r = 0$

which agrees with the result of ordinary differentiation. And if n and r be both negative integers, then

$$n_r = \frac{-n.-n-1\ldots \text{to } \infty}{r-n.r-n-1\ldots \text{to } 1 \text{ or } \infty} = \infty \text{ or } \frac{1}{r-n\ldots 1-n},$$

∞ holding when r is equal to or greater than n, and the finite value when $r<n$. This too is perfectly consistent with the common integration; for our theorem must of course give uncorrected integrals. Thus in the well-known $\int\frac{dx}{x}$ the common value $\log x$ is not the natural but a corrected value.

* This theorem, by changing differentials into integrals by a mere change of the sign of the exponent of the order, would induce us to term what we call integration, negative differentiation.

The

The natural value is $\frac{x^0}{0} = \infty$; but if this be corrected it becomes

$$\frac{x^0 - a^0}{0} = \frac{\{1+(x-1)\}^0 - a^0}{0}$$

$$= \frac{1 - a^0 + 0(x-1) + 0\frac{0-1}{2}(x-1)^2 + 0\frac{0-1}{2}\cdot\frac{0-2}{3}(x-1)^3 \ldots}{0}$$

$$= (x-1) - \frac{(x-1)^2}{2} + \frac{(x-1)^3}{3} \ldots$$

which is denominated the log. of x.

We may here observe that the properties of logarithms, and the same form to the function, would be found, if we seek the form of ψ so that

$$\psi . x^n \text{ may be } = n\psi x \qquad \text{(H)}$$

generally. For changing n into $\pm\, m$ and adding we have

$$\psi . x^n + \psi . x^{\pm m} = (n \pm m)\psi x = \psi . x^{n \pm m}$$

or

$$\psi . x^n \pm \psi . x^m = (n \pm m)\psi x = \psi . x^{n \pm m} \qquad \text{(I)}$$

Putting therefore $y = x^n$ and $z = x^m$ which, since m and n may be any independent quantities, may allow y and z the utmost independence, we find

$$\psi y + \psi z = \psi . yz \text{ and } \psi y - \psi z = \psi . \frac{y}{z} \qquad \text{(K)}$$

Moreover, differentiating (H) we get

$$x\psi' x = x^n \psi' . x^n = x^0 . \psi' . x^0 = \psi' . 1 = a$$

and consequently

$$\psi' x = \frac{a}{x} \qquad \text{(L)}$$

which is the well-known form of the differential coefficient of $a \times \log x$. From the assumed (H) it follows when $n = 0$ that $\psi . 1 = 0$; and the other principal properties of logarithms are evidently contained in (I), (K), (L).

If we investigate the problem under the still more general form of

$$\psi . x^n = n\psi_{,} x$$

and search the relation of the functions ψ, $\psi_{,}$ we should arrive at the same conclusion.

But to return: if n be a positive integer and r any non-integer, $\Delta^n . 0^n$ is finite and $\Delta^{n-r} . 0^{n-r}$ infinite, and of course $n_r = 0$. Therefore, *any non-integral differential of a positive integral power is nothing.* This curious property of fractional differentials may be otherwise proved, but my limits will not allow of minutiæ.

If n be a non-integer and $n-r$ a positive integer, the numerator of (9) is infinite, the denominator finite; and therefore

$$n^r = \infty .$$

It

It is also obvious from n_r in (9) that

$$d^{a-b+c\ldots} = d^{-b}\,d^{a}\,d^{c} = d^{c}\,d^{a}\,d^{-b} = d^{c}\,d^{-b}\,d^{a}$$

a well-known property.

I have before observed that our formula (9) gives only the uncorrected integral. If the correct and complete integral be sought when r is an integer it will be

$$d^{-r}.x^{v} + a_0 d^{1-r}.x^{0} + a_1 d^{2-r}.x^{0}\ldots a_{r-1} d^{0}.x^{0} \qquad (12)$$

in which a_0, a_1, a_2... are the arbitrary constants. And if r be a rational fraction $= \frac{m}{z}$, the complete integral will be

$$d^{-\frac{m}{z}}.x^{v} + a_0 d^{1-\frac{m-1}{z}}.x^{0} + a_1 d^{1-\frac{m-2}{z}}.x^{0}\ldots a_{v-1} d^{1-\frac{1}{z}}.x^{0} \qquad (13)$$

the arbitrary constants being such, if the problem requires it, as to neutralize the fractional differentiation on the integer power 0. We must therefore in considering (13) regard the several orders of d as referring to the powers of x only, and the numeral coefficients as comprehended in the arbitrary constants. This remark is of considerable importance towards a right understanding of the import of (13). It would otherwise appear that a successive differentiation of the order $\frac{1}{z}$ to $\frac{m}{z}$, which successively nulls the right hand terms and ultimately leaves as it should $= x^{v}$, is different from a single differentiation of the order $\frac{m}{z}$; whereas it ought to be and indeed is the same. For the $\frac{m}{z}$ differential of any term the $a_{y-1} d^{1-\frac{m-y}{z}}.\,x^{0}$ is actually

$$d^{\frac{m}{z}}.a_{y-1} d^{1-\frac{m-y}{z}}.\,x^{0} = a_{y-1} d^{1-\frac{m-y}{z}}\, d^{\frac{m}{z}}.\,x^{0}$$

which, since by our preceding $d^{\frac{m}{z}}.\,x^{0} = 0$ and $a_y d^{1-\frac{m-y}{z}}.\,x^{0}$ is finite, is null.

Hence it appears that the complete rth integral of any simple quantity x^{v} consists of as many terms as there are units plus one in the numerator of the order r, the order being in its lowest terms. Therefore when the order is irrational or imaginary the number of terms is infinite.

We may now easily deduce some interesting properties of the series (3) and (5), and show how to approximate to the value of the ratio $\frac{\Delta^{n}.0^{n}}{\Delta^{n-r}.0^{n-r}}$, when the factors run on to infinity

finity with unequal terms: we may also, from the developments of

$$f d\varphi(x,y) = f\left\{d_x + d_y\right\}\phi(x,y) \qquad (14)$$

f and ϕ implying any functions whatever and d_x, d_y differentials respectively relative to x and y, deduce some valuable consequences; but such speculations, to do them justice, would much surpass the reasonable limits of a mere sketch like this. I shall therefore now merely develop $d^{-r} u_x$, and proceed to general differences. Suppose in (14) $fd = d^{-r}$, and $\varphi(x,y) = x^\circ u_x$ then by our demonstration of the binomial theorem

$$d^{-r} u_x = 0_{-r} x^r u_x - r.0_{-r-1} x^{r+1} d u_x + r\frac{r+1}{2}.0_{-r-2} x^{r+2} d^2 u_x \ldots (15)$$

whatever be the value of r, supposing $dx = 1$. If $r = 1$ the expression coincides with Bernouille's. This is the *natural* integral; if the *complete* be sought it is found by merely adding the correction of (12) or (13), as r may be an integer or fraction to (15). This is the complete integral of any function of x whatever be its order found in the successive differentials of the given function. It however depends on the form of u_x whether this expression be very useful. Other and much more serviceable formulæ might be given, by considering the function as of two variables; but as they would lead me into discussions foreign to my present object, which chiefly respects non-integral differentiation, I shall not swell the paper with them.

On General Differences.

When the order of the difference is a positive integer, the formula (4) is probably as good as any that could well be given; but if the order be fractional or negative it is better to have recourse to (6) or (7). For instance, n being negative we have

$$\Delta^{-n}.x^v = \frac{x^v t^x}{(t-1)^n} + 2_n \frac{t^{x+1}\Delta.x^v}{(t-1)^{n+1}} + .3_n \frac{t^{x+2}\Delta^2.x^v}{(t-1)^{n+2}} \ldots * \quad (16)$$

when $t = 1$. But in this case it is obvious that all the members become infinite. Now I have already observed in negative differentials, that the integrals obtained by our process are uncorrected. The same is also true in negative differences. If we therefore partially correct the preceding expression by differentiating each term with respect to t in the

* I here employ 2_n, 3_n, .. to signify the binomial coefficients.

numerator

numerator and denominator, until the exponents in the denominator are reduced to 0, we shall have

$$\Delta^{-n}.x^{v} = \frac{x_n\, x^{v}}{n_n} + \frac{2_n(x+1)_{n+1}}{(n+1)_{n+1}}\Delta.x^{v} + \frac{3_n(x+2)_{n+2}}{(n+2)_{n+2}}\Delta^2.x^{v}.. (17)$$

employing the same notation for the differential coefficients as before, and ultimately putting $t = 1$. This expression, it is evident, always terminates when v is a whole positive number. It however depends on the properties of the differential numeral coefficients whether the value of the formula be nothing, finite, or infinite; but having considered these at some length in the general differentiation it would be tedious to go through a repetition in an application of them in this place. Most of the properties are very similar to and coincide with those for the integration of x^v, when n is a non-integer. If n be an integer, then

$$\Delta^{-n}.x^{v} = \frac{x...x-n+1}{1.2...n}x^{v} + 2_n\frac{x+1...x-n+1}{1.2...n+1}\Delta.x^{v} + 3_n\frac{x+2...x-n+1}{1.2...n+2}\Delta^2.x^{v}... \quad (18)$$

This expression terminates when v is a positive integer whatever be the value of x, and also when x is an integer from $< n-1$ to $-\infty$ whatever be the value of v; a property that I have not noticed in any other theorem for integration I have yet met with, and which may obviously be applied to some curious purposes.

The formula (18), though partially corrected, gives not the complete integral. What we have done is only to bring it from an infinite to a finite value. To have the complete integral difference requires a correction with the inferior natural integrals, similar to the correction in the negative differential. The complete integral is therefore

$$\Delta^{-n}.x^{v} + a_0\Delta^{1-n}.x^0 + a_1\Delta^{2-n}.x^0 + a_2\Delta^{3-n}.x^0 ... a_{n-1}\Delta^0.x^0 (19)$$

when n is an integer. Precisely analogous to (13) comes out likewise the complete integral difference when n is a fraction; and I therefore think it unnecessary to give it.

The transformation in (6) or (7) applies equally well whether we take x^v, or u_x any function whatever of x. Therefore by (17), (18), and (19) we shall have

$$\Delta^{-n}.u_x = \frac{x_n}{n_n}u_x + 2_n\frac{(x+1)_{n+1}}{n(n+1)_{n+1}}\Delta u_x + 3_n\frac{(x+2)_{n+2}}{n(n+2)_{n+2}}\Delta^2 u^x + ... (20)$$

the *natural* integral for any value of n; and

$$\Delta^{-n}u_x = \frac{x...x-n+1}{1.2...n}u_x + 2_n\frac{x+1...x-n+1}{1.2...n+1}\Delta u_x + ... \quad (21)$$

 the

the *natural* integral when n is a whole number; and

$$\Delta^{-n} u_x = (21) + a_0 \Delta^{1-n} . x^0 + a_1 \Delta^{2-n} . x^0 + \ldots a_{n-1} \Delta^{3-n} x^0 (22)$$

the *complete* integral n being a whole number.

Should $\Delta_a u_x = \frac{u_{x+a} - u_x}{a}$ we shall have

$$\Delta_a^{-n} u_x = \frac{(x-a(n-1))\ldots x}{1.2\ldots n} u_x + 2_n \frac{(x-a(n-1))\ldots(x+a)}{1.2\ldots(n+1)} \Delta_a u_x \ldots (23)$$

for the *natural* integral when n is a whole number and the common difference $= a$. And because

$$d^r u_x = \frac{(u_0 - 1)^r u_x}{0^r} = \Delta_0^r u_x \qquad (24)$$

if in (23) we put $a = 0$ we find

$$d^{-n} u_x = \frac{x^n}{1.2\ldots n} u_x + 2_n \frac{x^{n+1}}{1.2\ldots(n+1)} du_x + 3_n \frac{x^{n+2} d^2 u_x}{1.2\ldots(n+2)} \ldots \qquad (25)$$

which coincides with (15) when $r = n =$ a whole number.

We see from (24) and (25) that the calculus of differences, and the differential calculus are not merely similar, but one and the same in principle; the differences in the one case being finite and in the other nothing. Hence the reason of the strong similarity in the processes, though it should be observed that what is usually understood by the method of differences is in fact the calculus of functions.

It would be superfluous to make any observations on the remarkable simplicity and generality of these formulæ. Nor can it be expected that I should here notice the variety of purposes to which the double liability of (21) and (23) to terminate may be applied. These speculations may probably form a part of what I may have to say of the calculus to which I have already alluded.

LIV. *Some Notices concerning the Plants of various Parts of India; and concerning the Sanscrita Names of those Regions. By* FRANCIS HAMILTON, *M.D. F.R.S. & F.A.S. Lond. and Edin.**

AS it is my intention soon to publish, in various works on Natural History, the observations on the Botany of India which I made during my residence there, I wish to place on record an account of the opportunities which I enjoyed of making such observations, with the view of explaining to the botanist where he may find the various collections which I

* From the Edin. Phil. Trans. vol. x. part I.

made

made in different parts. I also wish to explain the geographical terms that I shall employ in giving an account of the places where I found each species. For this purpose I prefer using the ancient Sanscrita names, both as being more scientific, and as being more likely to remain permanent; for, after a lapse of many ages, they continue to be known to all Hindus of learning, while each new invasion or revolution sinks into immediate oblivion the mushroom appellations imposed by modern rulers, whether Muhammedans or Christians.

Immediately after my appointment to the Company's service on the Bengal establishment, I was sent with Captain Symes to the court of Ava, and, during the year 1795, I had an opportunity of seeing somewhat of the Andaman islands, with a good deal of the kingdoms of Pegu and Ava. The plants of the Andaman islands are nearly similar to those of Chatigang, of which I shall give a more full account. Those of Pegu nearly resemble those of the southern and eastern parts of Bengal, while those of Ava bear a stronger resemblance to the productions of Mysore. The reason of this seems to be, that the territory of Pegu enjoys much more copious rains than Ava, which, like the southern parts of what we call Hindustan, is a parched country, and, in order to bring rice to maturity, requires artificial irrigation by means of reservoirs or canals. On the way, however, between Pegu and Ava, where we approach the mountains bordering Arakan on the east, we had a vegetation much resembling that of Chatigang and of the mountains extending from thence along the eastern frontier of Bengal, which will be afterwards described. The plants which I collected during this journey were transmitted, together with a good many drawings, to the Court of Directors, and were given to Sir Joseph Banks, in whose collection they probably remain; but copies of most of the drawings, partly coloured, were preserved by me and deposited in the Company's library. I also preserved a copy of the notes, which I took on the spot, and this will be found in the same collection.

In 1796, 1797, and part of 1798 I was stationed at Lukhipur, in the south-eastern part of Bengal, and in the ancient kingdom of Tripura. My time was there much occupied in describing the fishes of the country; but I took many descriptions of plants, which are also deposited in the Company's library; but I did not preserve specimens. I corresponded, however, very frequently with Dr. Roxburgh, and transmitted to him whatever he thought would be acceptable, learning at the same time what both he and Kœnig called various plants.

In spring 1798, by the desire of the Board of Trade at Calcutta,

Calcutta, I visited the district of Chatigang, which, together with that of Komila, formed the chief part of the ancient kingdom of Tripura; and I afterwards skirted the hills of Komila, where the tribe of Tripura still maintains a kind of independence. Here I had a full opportunity of examining the splendid vegetation of the well watered districts of Further India (*extra Gangem*), which bounds the extensive Gangetic plain on the east, and extends south from what we call China to the ocean. It must be observed, however, that this Further India, as it has been called, is the proper China of the Hindus, from whom we derived the word, while what we name the Chinese empire, the Hindus call Maha China, or the Great China.

The largest portion of this Further India, or Southern China, is mountainous and well watered; but its mountains no where rise to an alpine elevation, and, owing to a copious supply of moisture, and a deep soil, are, in general, covered to the summit with lofty forests. I have already mentioned that a great part of the proper kingdoms of Pegu and Ava differs a good deal from the general appearance of the neighbouring countries, the former resembling more the southern plains of Bengal, and the latter the southern peninsula of India; but by far the greater portion of this Further India, in its vegetable productions, resembles Chatigang; and what Rumphius called *India Aquosa*, or the immense Eastern Archipelago, including the Andaman and Nicobar islands, may be considered as belonging to the same vegetable arrangement. Of this the most prominent feature is a tendency in trees of considerable size to twine round others, forming thus forests almost totally impervious. These twining trees, the *Funes sylvestris* of Rumphius, are often thicker than the human body, and extend to great distances, overwhelming the most lofty and vigorous woods; and so strong is the tendency to this kind of vegetation, that some even of the *Palmæ* (*Calamus*, L.), a tribe in general remarkable for erect stiffness, are here climbers, and, after overtopping the highest trees, again drop branches to the earth, which take root, and climb up the trees that are adjacent; and thus, with other thicker, though less powerfully armed climbers, form a mat which becomes almost impenetrable. This thick vegetation produces a delightful coolness, and preserves a moisture that encourages the growth of numerous and beautiful parasitical plants, *Filices*, *Aroideæ*, and *Orchideæ*; but renders the climate rather sickly to constitutions unaccustomed to such a moisture. In this fine region, the valleys between the hills are uncommonly fertile, and, being well watered, produce abundant crops of rice, the grand source of nourishment for the inhabitants, although the tuberous *Aroideæ* and *Dioscoreas*, both

both very nutritious, may be considered as the proper offspring of this territory, where they thrive with an uncommon vigour and variety. In this country, even the unoccupied wastes have a luxuriance of vegetation that renders them almost equally impervious with the forests; and grasses, mostly of the genus *Saccharum*, shoot up with a prodigious luxuriance and thickness. They generally exceed six feet in height, and often reach to twice that elevation.

The trees that are most common in this territory are of the orders of *Urticæ*, *Euphorbiæ*, *Terebinthaceæ*, *Magnoliæ*, *Meliæ*, *Guttiferæ*, *Sapotæ*, *Vitices*, and *Eleagni*, and, together with the *Palmæ*, *Bambusæ* and climbers, form the great features of vegetation, which are of a totally exotic appearance to the European, having scarcely any thing to recall the memory of his native scenery; yet still highly pleasing, not only from their novelty, but also from their beauty and grandeur. Notwithstanding this great difference of general appearance, several of the trees have an affinity with those of Europe, and the woods contain an *Æsculus*, and several *Querci* and *Coniferi*.

The specimens which I collected during this journey were transmitted to Sir Joseph Banks, in whose collection I saw them in the year 1806, and there they no doubt will still be found.

Soon after my return from Chatigang, I was removed to Baruipur, a station near Calcutta, where I chiefly employed my leisure in describing fishes. Still, however, I continued to collect whatever appeared rare for Dr. Roxburgh, especially during several journeys which I made through the great forests that occupy the islands formed by the estuaries of the Ganges. These dreary woods, half inundated by the tides, and skreened by banks of offensive mud, afford but little scope to the botanist. The variety of vegetables which they contain is by no means great; and the danger in attempting to collect them, by landing where tigers are so numerous and ravenous, is very great.

I believe, however, that in various journeys which I made between Calcutta and Lukhipur, and from Baruipur, through these woods and islands, forming part of the ancient kingdoms of Vanga, Upavanga, and Angga, I had an opportunity of describing most of their vegetable productions. Mangroves, of various kinds, including *Rhizophora*, *Ægiceras*, *Avicennia*, *Sonneratia*, and *Heritiera*, especially the latter, form the predominant feature of these woods; but they are ornamented with curious *Convolvulaceæ* and *Apocineæ*, with many parasitical *Filices*,

Filices, and some elegant *Lycopodiums* and *Lichens*, not remarkable indeed for variety, but of great size and beauty.

The cultivated parts of this Delta of the Ganges, as it has been called, are not more favourable to the botanist than the wastes. The plough or hoe occupies almost every spot, one rice-field succeeds another; and the houses are buried among groves of *Mangifera*, *Artocarpus*, and *Bambusa*, intermixed with *Palmæ*, and are only kept above water by being raised on the banks thrown up by digging ponds. In this territory the wastes are generally covered with reedy grasses, almost as lofty as those of Tripura. The whole aspect, indeed, of the country and of its vegetation, is strange and foreign to an European, unless to a Hollander. For four months in the year every field swarms with fish, and at all times the only conveyance is by boats.

During my stay in this part of the country, I made few botanical observations, except by communications with Dr. Roxburgh. I however transmitted a few descriptions and drawings to Sir James Edward Smith, with whom they still remain.

During the year 1800 I was employed by the Marquis Wellesley to examine the state of the country which he had lately taken from Tippoo Sultan, and of the province which Europeans call Malabar. I landed at Madras (Chinapataná of the natives), and travelled through the territory belonging then to the nabob of Arcot, which Europeans call the Carnatic, but it is the Draveda of the Hindus, bounded on the south, at the mouth of the Kaveri, by Chola, which Europeans call Tanjore, and to the north by Andhra, the sea-coast of which by Europeans is usually called the Circars, as having once been divided into five districts (Circar), which were early ceded to Europeans by the Muhammedan princes of Andhra or Tailingana. The coasts of Chola, Draveda, and Andhra are usually included by Europeans under the denomination of Coromandel, a name totally unknown to the natives, who consider it as English, and from which we have several plants named *Coromandeliana;* as from the English word Madras, with the addition of Patana (city), we have *Maderaspatana*, as if plants grew in the streets. Both names should be avoided as inconveniently long, as well as devoid of meaning in any language.

On leaving Draveda, and ascending to the elevated region lately under the dominion of Tippoo Sultan, I entered the ancient Hindoo territory, called by them Karnata (Latinè *Carnata*), but usually known to Europeans by the name of Mysore,

Mysore, from the town where its princes for some generations resided. Having examined this and the skirts of the interior of Andhra, I descended again to the low country by the south, and examined the country west from Chola, which the natives call Chera or Cheda, but which Europeans, from a town in it, call Coimbetore (Coiamatura). Chera as well as Chola is bounded on the south by the country which the natives call Pandiya, extending from near the Kaveri to the Southern Ocean. The northern parts of this towards Chera I had an opportunity of examining. The vegetation of all these countries is nearly similar. The elevation of Mysore above the others, although probably about 3000 feet of perpendicular height, produces no great change. The temperature is no doubt somewhat lower, and more agreeable to European feelings; but the aspect of the upper country is not materially different from that of the lower. Both labour under a scarcity of rain, so that artificial irrigation from reservoirs or canals is necessary for the production of rice, which, in the low country especially, is the staple article of food, although both there and in the higher country the rainy season produces crops of miserable small grains, (such as *Eleusine Corocanus*, *Panicum Italicum*, and *Panicum miliaceum*,) that are used by the natives as a succedaneum for rice. These crops have little of an European appearance; nor do the orchards and gardens heighten the resemblance, The fruit-trees round the villages consist chiefly of the *Mangifera*, *Citrus*, *Bassia*, *Artocarpus*, *Eugenia*, *Elate*, and *Borassus*, while the kitchen-gardens require to be watered by machinery from wells. The general appearance of the country is sterile, the rock projecting in a great many places, while during the greater part of the year the grass is entirely parched up from want of moisture; and even in the rainy season the grass is not longer than is usual in Europe. In the woods the trees are still more stunted than those of Europe, and consist in a large proportion of wild prickly dates (*Elate sylvestris*) and *Bambusæ*, with trees of the *Leguminosæ*, especially such as have prickles, and of the *Rhamni*. Even the thickets consist chiefly of bushes of the *Leguminosæ*, and of the *Rhamni* and *Caparides*, almost all armed with prickles or thorns, while the fences are chiefly of naked *Euphorbiæ* (*Antiquorum* and *Tirucalli*). The most common trees, besides the *Leguminosæ* and *Rhamni*, belong to the tribe of *Eleagni* and the genus *Grewia*: and the most common herbage consists of small *Cyperus*, *Scirpus*, *Andropogon*, *Convolvulaceæ*, *Acanthaceæ*, and *Leguminosæ*, especially *Hedysarum*, *Crotolaria*, and *Indigofera*; so that the vegetables have little in common with those of Europe, especially of its northern

 parts.

parts. With the more barren parts of southern Europe there is more resemblance, the *Rhamni* and *Caparides* being common to both.

After examining these countries of rigid vegetation, as it may be called, I passed through the gap in the Animaliya, or Elephant Mountains, and entered the province called Malabar by Europeans, but Kærula and Malayala by the natives. These, indeed, consider Malabar as an English word, meaning the whole sea-coast between Cape Comorin and Surat, which seems to be the fact. We ought, therefore, to call the province of Malabar by one or other of the native appellations. The territory called Kærula by the natives extends from the southern extremity of India to almost the latitude of 12½ degrees north: but this includes a portion of the English province of Canara; and it extends from the summits of the mountains to the sea. In its vegetable productions and appearance it more resembles Chatigang and the mountains of Farther India than the adjacent territory of rigid vegetation; but it is better cultivated, contains more plantations, especially of *Palmæ*, and, the rock projecting more, the vegetation is not quite so luxuriant. It has, however, perhaps still less of an European appearance, none of the *Amentaceæ* nor *Coniferæ* being found in its woods. The Dutch, however, have introduced many fine trees from the Eastern Islands, and the Portuguese some from the West Indies, both of which give a considerable variety to its plantations; and few countries possess a vegetation so elegant, prospects more grand and beautiful, and a climate more genial: its highest mountains, although of considerable height, perhaps 6000 feet perpendicular, have nothing of an alpine appearance, but produce a moisture and coolness that extends a more vigorous vegetation to the adjacent country above.

Nearly connected with Kærula, and little different from it in vegetable productions, is Ceylon, the Taprobana of the Romans and the Lanka of the ancient Hindus. In 1815, I had an opportunity of a cursory examination of its southern end, and saw sufficient to indicate that in general aspect at least it does not materially differ from Malayala.

North from Kærula, and, as I have said, including a portion of it, is the extensive English province of Canara, a word of doubtful origin, and supposed by the natives to be English. The Hindus divided it into four territories: 1st, Part of Kærula or Malayala, extending to about 12° 28′ north latitude; 2d, Tulava, extending from thence to about 13° 35′ N.; 3d, Haiva or Haiga, extending to about 14° 38′ N.; and 4th, Kankana (Latinè *Cuncana*), extending to the Portuguese territory of

of Goa; but this, as well as all the sea-coast to near Bombay, are included in the territory which the Hindus call Kankara. These countries, like Malayala, extend from the summit of the mountains to the sea, and scarcely differ in appearance or vegetable productions from that territory; but they are rather hotter and drier, and their vegetation is rather less vigorous, approaching more nearly to the rigid thorny nature of that prevailing towards the east.

The specimens of plants which I procured during this journey suffered much by the carelessness of those who were entrusted in conveying them from the ship to Calcutta; but such as they were, they were given to Sir James Edward Smith, together with a good many drawings, and both remain in his collection. The notes which I took have been deposited in the Company's library. Some duplicate specimens were given to A. B. Lambert, Esq.; and I think that Sir James Edward Smith has a copy of the notes: of this, however, I am not certain.

Soon after my return from the south of India, I was sent to Nepal along with the embassy conducted by Captain Knox. Having proceeded by water to Patna, I passed by easy stages and with many halts through the ancient territory of Besala, now called Sarun; and through a portion of Mithila, now called Tirhut. There I carefully examined and collected such plants as were in flower; and, on the 1st of April 1802, I ascended into Nepal, where I remained nearly twelve months, delighted with the variety, beauty and grandeur of its vegetable productions, of which I procured many specimens, descriptions and drawings; all of which I gave to Sir James Edward Smith, only reserving specimens, where there were duplicates, for Mr. Lambert. I afterwards had an opportunity of procuring many specimens from the same quarter, and of making many observations on these plants, which I may have occasion to use under the disagreeable circumstance that I may have described the same plant under different names, among those given to James Edward Smith, and among those which I afterwards procured; but under the circumstances already mentioned this was unavoidable. For an account of the appearance of the vegetables in this interesting region I may refer to the account of Nepal which I have published.

Soon after my return to Calcutta in 1803, I was appointed surgeon to the Governor-general; and the leisure I then had for the study of natural history was chiefly employed in superintending the menagerie founded by the Marquis Wellesley, and in describing the animals there collected. I returned to England with this distinguished nobleman in the end of 1805,

and in 1806 was appointed by the Court of Directors to make a statistical survey of the territory under the presidency of Fort William, usually in Europe called Bengal; but containing many extensive regions besides Bengal, taking that even in the most extended sense of the Mogul province of the name; for in Hindu geography, Vanga, from whence Bengal is a corruption, is applied to only the eastern portion of the Delta of the Ganges, as Upavanga is to the centre of this territory, and Angga to its western limits.

I commenced this survey after the rainy season of 1807, with the English district of Dinagepore (Dinajpura), forming part of the ancient kingdom of Matsiya, bounded by the Mahananda on the west, by the Korataiya (Latinè *Coratæa*) on the east, by the mountains on the north, and by the Padma or eastern branch of the Ganges on the south. This district is not very favourable for the botanist, being in general highly cultivated; but its southern parts, especially round the ancient city of Purua are woody, and yielded a considerable increase to my collection.

In spring 1808, having finished the survey of Dinagepore, I passed through the English district of Rungpur (Ranggapur), the Kamrupa of the ancient Hindus; and having examined the north-eastern wastes of that territory, where I added much to my botanical stores, I halted for the rainy season at Goyalpara (Latinè *Goalpara*). This place, situated at the northern extremity of the mountainous district which bounds the Gangetic plain on the east, afforded me most ample employment as a botanist, producing a variety of beautiful and rare plants, almost equal to that of Nepal; and, with my journeys to Ava and Chatigang, enabled me to form a proper estimate of the vegetable productions of Farther India (*ultra Gangem*), the China of the Hindus, and which I have already described.

With the fair weather of 1808 I recommenced the survey of the Rungpur district, where I found an excellent field for a botanist, as it contains many wastes. As the rainy season of 1809 approached, I retired to a house near the town of Rungpur, and there continued in a situation not very favourable for a botanist, until I had time left only to convey me to Purneah (Puraniya), before the fair weather of 1809 should commence.

The English jurisdiction of Purneah (Latinè *Purania*) forms a part of the ancient Hindu kingdom of Mithila, with a small portion of Angga around the ruins of Gaur; but my journey during the dry season added little to my botanical stores. This, however, was amply recompensed by my stay, during the rainy

rainy season 1810, at Nathpur, on the frontier of the Kiratas, or Ciratas, subject to Nepal; from whence, as well as from the forests in the northern parts of Mithila, I procured a great variety of rare and curious plants.

In autumn 1810, so soon as the weather cleared, I proceeded to the district of Boglipore (Bhagulpur), the eastern part of which is included in the ancient Hindu kingdom of Angga, while its western portion is in Magadha; and the portion on the northern banks of the Ganges is partly in Angga, partly in Mithila. The greater portion of this district being waste, was very favourable to me as a botanist; and I had here an opportunity of extending my knowledge of the rigid vegetation of the Vindhiyan mountains, which the Hindus consider as bounding the Gangetic plains on the south, and as extending from the southern banks of the Ganges to the Southern Ocean. These hills are here much lower than the parts of the same mass which I examined in the south; but their vegetable productions are nearly the same, and have a similar rigid thorny appearance; but, the rains being more copious, the vegetation is not quite so much stunted, although it is very far from being so luxuriant as that towards the east or north.

The rainy season 1811 I passed at Mungga, where the vicinity of the hills gave me a considerable increase to my stock of plants, and I employed a Hindu physician, not deficient in learning, to point out the plants which he considered officinal, and to give me both their Sanscrita and Hindu names, which I compared with those given to the same plants by the ignorant people who collect and vend drugs.

In the following dry season 1811-12 I examined the jurisdictions subject to the magistrates of the cities of Patna and Gaya, both included in the ancient kingdom of Magadha, which for many centuries before the Muhammedan invasion was considered the chief seat of Hindu power and glory, so that its princes were indifferently called kings of Magadha and of Bharatkanda, or the Land of Virtue, the name by which the Hindus fondly call the territory occupied by their race, the descendants of Brahma. In these districts I had a further opportunity of making myself acquainted with the rigid vegetation of the Vindhiyan mountains, and, during my stay at Patna, in the rainy season 1812, I extended my knowledge of the officinal plants of India, by consulting the same physician and the druggists of Patna.

In the dry season 1812–13 I examined the jurisdiction under the magistrate of Shahabad, forming a great part of the ancient Hindu kingdom of Kikata (Latinè *Cicata*); and here I completed my knowledge of the vegetation of the Vindhiyan mountains,

mountains, which, the further west I proceeded, rose to a greater elevation, were more rocky, and communicated to their vegetation more and more of the rigid and thorny nature of that produced on the arid hills and mountains of Draveda, Kamata, and Chera.

Soon after the rainy season of 1813 commenced, I embarked at Chunar, and proceeded up the Ganges and Yamuna (*Jomanes* Plinii) or Jumna to Agra; and thus had an opportunity of examining the plants on the banks of these rivers, passing along a portion of the ancient kingdom of Malava (Malwa) on the east of the Yamuna river, near the Ken (*Cainas* Plinii) and Chumbul rivers, and then proceeding through the centre of the ancient kingdom of Kuru, which, in the earlier part of the Hindu government, was the chief seat of power and glory, restored to it afterwards by the Muhammedan conquest, and only lately restored to Angga by British valour and prudence; for in the time of Alexander, Angga was no doubt the chief seat of Hindu power, as Palibothra seems to have been seated opposite to Rajamahal, in Angga, although on the skirts of Magadha, which in latter times was the great seat of authority.

Before the end of the rainy season I returned down the rivers, and ascending the Gagra, entered the district of Gorakhpur, forming a considerable portion of Cosala, the territory of the powerful Family of the Sun, who reigned at Oude (Ayudhiya). During the dry season 1813–14 I remained in the district of Gorakhpur, where I made large additions to my botanical observations, both from the forests of the country, and from the neighbouring parts of Nepal, from whence I procured many plants.

When the rainy season commenced I again embarked, and proceeded up the Ganges to Futehgar, where I had again an opportunity of examining the vegetable productions of the ancient kingdom of Kuru, through the centre of which the Ganges passes; for it includes both banks of the Ganges and Yamuna, being bounded on the east by Kosala, and on the west by Pangchala, now called the Punjab, or the country watered by the five rivers joining the Indus from the north-east.

Having thus examined a considerable portion of the Gangetic plain, always considered the proper seat of the Hindu race, descended from a colony of civilised persons calling themselves sons of Brahma, who in the earliest ages settled at Vithora (*Betoor* Rennell), and gradually extended their power over what is now called Hindustan, I shall proceed to give some general account of the vegetation of this fertile tract, which, without any thing that can be called a hill, extends

from

from the Indus to the Eastern Ocean, and from the Vindhiyan to the Himaliya mountains.

This plain, extending in length about fourteen degrees of longitude, in the middle latitude of 25°, and in breadth from two to four degrees of latitude, seems to derive a large proportion of its vegetation from the neighbouring hills; but grasses, especially *Bambusa*, *Saccharum*, *Andropogon*, *Apluda*, and *Panicum*, together with the allied tribes of *Cyperoideæ*, form a larger and more marked feature than trees or shrubs. On the whole, the rigid and thorny vegetation of the Vindhiyan mountains seems more suited for the plain than the more ornamental vegetation of either the Eastern or Himaliya mountains. Near both these, however, their plants have made considerable encroachments, and communicate a change of appearance to the adjacent plains, especially towards the east, where the air is vastly cooler and moister than further west.

I have already mentioned the appearance of the Gangetic Delta, which on the whole has a strange and exotic appearance to the European traveller. As we advance, however, to the north, and still more as we proceed west, notwithstanding the intense heats of the summer, the vegetation appears more of an accustomed form. Wheat, barley, pease and rape-seed form by far the largest proportion of the crops, and we observe fields of potatoes and carrots, while the *Palmæ* and *Bambusæ* disappear from the plantations; and the gardens produce the vine, the fig, the apple, and the plum, with many flowers common in Europe, and the thickets contain much of the wild rose. Still, however, even in Kuru, the *Mangifera*, the *Eugenia*, the *Calyptranthes*, the *Fici* (*religiosa* and *bengalensis*), the *Rhamni*, and the exotic crops produced in the rainy season (*Oryza*, *Holcus*, *Panicum*, *Paspalum*, *Dolichos*), with the want of the *Coniferæ* and *Amentaceæ* in the plantations, remind us sufficiently that we are not in Europe.

I now was exhausted by a long continued exertion, the observation of plants making but a small part of my duty, and I required to pass the remainder of my days at peace in my native climate, I accordingly returned to Calcutta to prepare for my journey; and in the mean time, on the death of Dr. Roxburgh, took charge of the botanical garden, having been appointed his successor by the Court of Directors. While preparing for the journey, I was deprived by the Marquis of Hastings of all the botanical drawings which had been made under my inspection during my last stay in India, otherwise they would have been deposited, with my other collections, in the library at the India House. By this ill-judged act of authority, unworthy of this nobleman's character, the drawings will

will probably be totally lost to the public. To me, as an individual, they were of no value, as I preserve no collection, and as I have no occasion to convert them into money.

In February 1815 I embarked for Europe, and in September presented my whole collections to the Court of Directors, with an order from the Lords of the Treasury for their being delivered free from duty,—an order which was granted with the utmost liberality and urbanity.

LV. *Outline of general Methods for the Development of certain Branches of Analysis.* *By* A CORRESPONDENT.

To the Editor of the Philosophical Magazine and Journal.

Sir,

ALTHOUGH the superiority of analytical methods of inquiry is now generally recognised, and such methods almost universally adopted, it is still frequently to be observed, that the change which has thus been effected in many investigations is more in the mere *form* than in the *spirit*. The essential character of the algebraic analysis consists in the extreme generality with which it regards every object it contemplates. It is this which raises it so infinitely above the investigations of geometry. These are necessarily confined to the contemplation of particular cases, and proceed by methods which might almost be termed *tentative*. Now the mere bestowing upon such methods the appearance of algebraic investigation, or the mere substitution of symbolic characters for words, does not constitute an analytical procedure. Before the subject can partake of the true spirit of analysis, tentative methods must be banished entirely—the *reasoning* as well as the *result* must be generalized—and all the widely different processes by which the particular truths of one family were formerly obtained, must be condensed into one sweeping march of symbolic transformations. I have said that this spirit of generality is not yet bestowed upon many investigations *professedly* analytical; and I refer in proof of my assertion to the common modes of treating the arithmetic of sines. The inquirer leads us on, throughout the elementary formulæ, by an algebraic synthesis, which like the geometrical appears natural, and is easily followed, so long as it merely regards the *elementary* formulæ; but the instant that he passes beyond the mere elements, and attempts to develope the remoter series, he is compelled to resort to methods of a varied and tentative character, that are quite unknown in a truly analytical inquiry.

quiry. This may perhaps arise from a desire to develope the subject in an entirely elementary manner; but it should be recollected, that the arithmetic of sines, or any portion of geometry, when treated analytically, is *not* properly an *elementary* subject. It is decidedly transcendental, and the whole resources of the calculus may be legitimately applied in its development. If it be conceived that such a mode would render one important part of practical mathematics of difficult acquisition, I answer, that practical subjects, considered *as such*, must always be developed in a manner correspondingly elementary; but if they are considered *as portions of science*, there is no reason why they should not take the places which of right belong to them in the course of its regular development. The fact that mechanics must be taught in as elementary a manner as possible, in many institutions in this kingdom for instance, is no argument against the importance of those refined analytical investigations of the doctrines of equilibrium and motion: and neither should a similar necessity deprive geometry of a similar treatment. If then two grand transcendental subjects were placed side by side, as they ought to be, and alike subjected to the operation of the most recondite as well as the most elementary relations of abstract magnitude, then might we expect a renovation in geometry similar to what has already been effected in mechanics, and a final exclusion of every particular or tentative process. It is my intention to attempt, in the course of a few papers, to exhibit the renovating and extending effect of general methods upon the arithmetic of sines. I hope to reduce every formula now known in this branch of analysis to a particular case of some extensive class of expressions, derivable by the mere mechanical modification of a still more general method; and thus to enable the humblest calculator to develope into series of any form, an infinite variety of finite trigonometric functions, almost as rapidly as he can write them down.

The arithmetic of sines occurs very early in the course of a purely analytical system of geometry. It was while delineating such a system that it came before me, in the form in which I would present it; and I allow it to retain the marks of its origin, by giving it as a portion of an extensive inquiry. Several facts are of course involved, with which the analysis of the previous equations easily furnished me: and the omission of these previous inquiries has compelled me to conform to the common modes of writing, more closely then I would otherwise have done.

The fundamental experiment which led to the definition of

 ϕ in

φ in the general equation furnishes us immediately with the particular one

$$x = \phi\,(y, \mathrm{x}, \mathrm{y}, \gamma)$$

or

$$\frac{x}{y} = \phi\left(\frac{\mathrm{x}}{\mathrm{y}}, \frac{y}{\gamma}\right)$$

If we suppose, for the sake of simplicity, that y is the right angle or the unit of angles, and y the unit of lines, we have

$$x = \varphi\,\mathrm{x}.$$

From the same triangle may be obtained in a precisely similar manner $z = \psi\,\mathrm{x}$.

The object of our inquiries is to ascertain the forms of φ and ψ. Let us then investigate their factors, or, in other words, the values of x that reduce them to zero. It may be easily discovered that x, and consequently $\varphi\,\mathrm{x}$, becomes 0 when, and only when,

$$\begin{aligned} \mathrm{x} = {} & 0 \\ & \pm\,\pi \\ & \pm\,2\pi \\ & \ldots\ldots\ldots \\ & \pm\,n\pi \\ & \ldots\ldots\ldots \end{aligned}$$

and likewise that z or $\psi\,\mathrm{x}$ vanishes when

$$\begin{aligned} \mathrm{x} = {} & \pm\,\frac{\pi}{2} \\ & \pm\,\frac{3\pi}{2} \\ & \ldots\ldots\ldots \\ & \pm\,\frac{2n+1}{2}\,\pi \\ & \ldots\ldots\ldots \end{aligned}$$

Whence

$$x = \varphi\,\mathrm{x} = a\,.\,\mathrm{x}\left\{1 - \frac{\mathrm{x}^2}{\pi^2}\right\}.\left\{1 - \frac{\mathrm{x}^2}{2^2\pi^2}\right\}.\left\{1 - \frac{x^2}{3^2\pi^2}\right\}.\,\&\mathrm{c}.$$

$$z = \psi\,\mathrm{x} = \alpha\,.\left\{1 - \frac{4\mathrm{x}^2}{\pi^2}\right\}.\left\{1 - \frac{4\mathrm{x}^2}{3^2\pi^2}\right\}.\left\{1 - \frac{4\mathrm{x}^2}{5^2\pi^2}\right\}.\,\&\mathrm{c}.$$

and since z or $\psi\,\mathrm{x} = 1$ when $\mathrm{x} = 0$, we have $\alpha = 1$.

The constant a is evidently the ratio of evanescent $\phi\,\mathrm{x}$ and x, which we must therefore determine. Previous investigations would have shown us that circular arcs may be adopted for angles, and also that the angle of a semicircle is a right angle. Now if a chord moves round the extremity of a diameter, the nearer it approaches the diameter, it the more nearly equals it. When the angle formed by them is evanescent, the chord and diameter are equal, and the evanescent chord which joins their extremities is at right angles to the diameter. But this evanescent chord is merely the ultimate element of the arc: hence is the

the evanescent arc at right angles to the diameter; and hence also must it coincide with the evanescent x or φ x. Consequently $a = 1$. The factorial expressions may, therefore, be developed into the following series

$$\varphi x = x - A_3 x^3 + A_5 x^5 - A_7 x^7 + \&c.$$
$$\psi x = 1 - A_2 x^2 + A_4 x^4 - A_6 x^6 + \&c.$$

where A_2 A_3 A_4 &c. are known functions of π and numbers.

The analysis of ϕ and ψ might now be deemed complete; but there are evident means of reducing the series to still simpler forms. The circumstances which regulate the invariability of triangles give us the following formula of connexion between ϕ and ψ, which, it is likely, will lead to important consequences.

$$\phi \left\{ \pm \frac{2n+1}{2} \pi + z \right\} = \pm (-1)^n \psi z. \qquad (1)$$

The development of φ (x + z) is

$$\phi x + \varphi' x . \frac{z}{1} + \varphi'' x \frac{z^2}{1.2} + \phi''' x \frac{z^3}{1.2.3} + \&c. \qquad (2)$$

And it is plain that when this series becomes equivalent to $\pm \psi z$, the coefficient of z or $\phi' x$ must be equal to zero. This is the case when

$$x = \pm \frac{2n+1}{2} \pi$$

which expression must therefore include at least one class of the factors of $\varphi' x$, or of the series

$$1 - 3A_3 x^2 + 5A_5 x^4 - 7A_7 x^6 + \&c.$$

But it includes all the factors of ψ x; and since ϕ'x and ψ x are *similar* functions, it now follows that if they differ in any respect, $3A_3$ will differ from A_2. An inspection of the known composition of these quantities, however, instantly shows us that they are equal; consequently

$$\varphi' x = \psi x$$

and by applying this to relation, (1), we likewise obtain

$$\psi' x = - \phi x$$

These two relations determine all the derivatives of ϕ and ψ, and if x is put $= 0$ in developments similar to development (2), we immediately obtain the simplified series

$$\phi x = x - \frac{x^3}{1.2.3} + \frac{x^5}{1.2.3.4.5} - \&c. \ldots$$

$$\psi x = 1 - \frac{x^2}{1.2} + \frac{x^4}{1.2.3.4.5} - \&c. \ldots$$

whence the important forms

$$\phi x \text{ or } \sin x = \frac{e^{x\sqrt{-1}} - e^{-x\sqrt{-1}}}{2\sqrt{-1}}$$

$$\psi x \text{ or } \cos x = \frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}$$

or

or if for the sake of convenience $e^{x\sqrt{-1}}$ be put $= z$, we have

$$2\sqrt{-1}\ \sin x = z - z^{-1}$$

$$2\ \cos\ x \qquad = z + z^{-1}.$$

These trigonometric functions being now reduced to exponential expressions are of course *given*, and their various properties may be deduced by mere analytical artifice. It were out of place for me to waste a moment's time in the deduction of the simplest forms of their combination. I shall regard them in the course of the following investigations as known, and hasten to develope general methods for the discovery of the more distant and difficult formulæ. The sine and cosine may be combined together into series of an infinite variety of forms; but the series whose terms involve either these functions of the ascending multiples of the arc, or the ascending powers of the functions of the simple arc, occur most frequently in practice, and are consequently of the highest interest. Let us then in the first place investigate the constitution of series of the following forms

$$A + B\cos x + C\cos 2x + D\cos 3x + E\cos 4x + \&c.$$

$$A + B\sin x + C\sin 2x + D\sin 3x + E\sin 4x + \&c.$$

Happily the investigation does not present the slightest difficulty. Each term of the two series is reducible to the form

$$2\sqrt{\pm 1}\ N\,(z^n \pm z^{-n}) \qquad (3)$$

and the summation of the series themselves is thus dependent upon the summation of such a series as

$$\phi z = A' + B' z + C' z^2 + D' z^3 + \&c.$$

When the coefficients A, B, C, D form either a recurring progression, or are coefficients of any known transcendental series, the infinite expressions may be summed at once. A variety of *random* expressions too may be found with equivalent expressions in series. ϕz has merely to represent a function of z developeable according to rising integral powers; and if either diminished or increased by ϕz^{-1}, a slight modification of the arising coefficients will give their values when the series correspond to given finite trigonometric quantities. Thus the equivalence of ϕz to $\frac{1}{1 \pm z}$ and $\log(1 \pm z)$ gives us the seven series

$$\tfrac{1}{2} = \cos x - \cos 2x + \cos 3x\ \&c.$$

$$-\tfrac{1}{2} = \cos x + \cos 2x + \cos 3x\ \&c.$$

$$\tfrac{1}{2}\tan\frac{x}{2} = \sin x - \sin 2x + \sin 3x\ \&c.$$

$\tfrac{1}{2}$ cot

$$\tfrac{1}{2} \cot \frac{x}{2} = \sin x + \sin 2x + \sin 3x \text{ \&c.}$$

$$\log (1 \pm \cos x) = - \log 2 \pm \tfrac{2}{1} \cos x - \tfrac{2}{2} \cos 2x \pm \tfrac{2}{3} \cos 3x \text{ \&c.}$$

$$\frac{x}{2} = \sin x - \frac{\sin 2x}{2} + \frac{\sin 3x}{3} - \text{\&c.}$$

$$\frac{\pi - x}{2} = \sin x + \frac{\sin 2x}{2} + \frac{\sin 3x}{3} + \text{\&c.}$$

By this simple means, a vast number of different processes may be reduced to one transformation; and were proof wanting of the simplicity as well as unity which an attention to it bestows on many investigations. I would compare the above with the investigations of the series for $\log (1 \pm \cos x)$ given by Euler and Lacroix in their great works on the Integral Calculus.

But all this is easy and comparatively trifling. The great problem before us requires the development of any function of z, B_z into a series whose terms are of the form (3). This may evidently be accomplished by decomposing B_z into the sum or difference of two similar functions, one of z and the other of z^{-1}, or by solving the functional equation

$$\varphi z \pm \varphi z^{-1} = B_z$$

I will not enter into the general solution in this paper, but will prepare the way for several methods of development which I mean to explain afterwards by the full consideration of a particular case. Let us investigate the general form of ϕz when

$$B_z = 0;$$

that is, let us determine the two classes of functions which enjoy the properties

$$\phi z = \phi z^{-1}$$

$$\varphi z = - \varphi z^{-1}$$

The first of these equations simply indicates that ϕ is not changed by the substitution of z^{-1} for z; and since z^{-1} and z are convertible into each other by mutual substitution, it is plain that this characterizing condition will be fulfilled by supposing φ a *symmetrical* function of z and z^{-1}. It is evident it can be fulfilled in no other manner. Hence if f represent any arbitrary form, we have for a *general* solution

$$\phi z = f(\overline{z}, \overline{z^{-1}}). \qquad (4)$$

Again, φz in the second equation must for similar reasons be a symmetrical function of z and z^{-1}, multiplied by some other arrangement of them which changes its sign when the quantities

tities are transposed. Whence an equally general solution for the second case

$$\phi z = f(\overline{z, z^{-1}}) . (z - z^{-1}) \ldots \qquad (5)$$

By means of these two expressions we may obtain as many series as we please of the sines and cosines of multiple arcs whose values are 0. But before illustrating the method by any particular case, let us see if we can connect with each case any *class* of series of a similar description. It is evident that if we could derive a series of functions of the character

$$\phi_n z \pm \phi_n z^{-1} = D\left(\phi_{n-1} z \pm \phi_{n-1} z^{-1}\right) \qquad (6)$$

where $\phi_n z \pm \phi_n z^{-1} = D^n\left(\phi_0 z \pm \phi_0 z^{-1}\right)$

we should have a new and extensive order of trigonometric functions hanging upon each form assumed by D and ϕ_0 or ϕ. Not to complicate the matter further than is necessary for the purposes of illustration, let us give D a particular form, and determine D^n in series by a process which, although particular, contains the elements of treatment that will be applicable to every case. The values of $\phi z \pm \phi z^{-1}$ as characterized in equations (4) and (5) are favourable to the determination of D^n when D is an integral. Let the formula of derivation from ϕz in the first instance, as characterized by equation (4), be

therefore $$\phi_n z = \int \psi z \, d z \, \phi_{n-1} z \qquad (7)$$

arising from $$\phi_1 z = \int \psi z \, d z \, \phi z$$

And let us modify $\psi z \, d z$ so that the condition (6) may be fulfilled by it. Substituting z^{-1} for z in equation (7) we have

$$\phi_n z = \int \psi z^{-1} \, d z^{-1} \, \phi_{n-1} z^{-1}$$

whence

$$\phi_n z \pm \phi_n z^{-1} = \int\left(\psi z \, d z \, \phi_{n-1} z \pm \psi z^{-1} \, d z^{-1} \, \phi_{n-1} z^{-1}\right)$$

where it is evident the requisite condition will be fulfilled if

$$\frac{\psi z^{-1}}{z^2} = \psi z \qquad (8)$$

Supposing then that ψz is thus characterized, the formula of derivation becomes

$$\phi_n z \pm \phi_n z^{-1} = \int \psi z \, d z \left(\phi_{n-1} z \mp \phi_{n-1} z^{-1}\right) + u_n c$$

noting by $u_n c$ the constant arising from integration. From this formula we immediately derive by successive substitution

$$\phi_n z \pm \phi_n z^{-1} = \int \psi z\, dz \left\{ \int \psi z\, dz \left(\phi_{n-2} z \pm \phi_{n-2} z^{-1} \right) + u_{n-1} c \right\} + u_n$$

$$= \int \psi z\, dz \left(\phi_{n-2} z \pm \phi_{n-2} z^{-1} \right) + u_{n-1} c \,.\, \frac{\int \psi z\, dz}{1} + u_n$$

$$= \int \psi z\, dz \int \psi z\, dz \left(\phi_{n-3} z \mp \phi_{n-3} z^{-1} \right) + u_{n-2} c \,.\, \frac{(\int \psi z\, dz)^2}{1.2} + u_{n-1} c \,.\, \frac{\int \psi z\, dz}{1} + u_n$$

&c.

The law of this derivation is immediately obvious, and it may be conveniently expressed in an abbreviatory symbolical form. If Φ be taken to represent $\int \psi z\, dz$, and $u^n_n c$ be conceived to signify $u_{n-2} c$ we shall have

$$\phi_n z \pm \phi_n z^{-1} = u_n (c + \Phi)$$

where since $\phi z - \phi z^{-1} = 0$, $u^n_n c$ and all higher orders of the derivatives of $u_n c$ must be 0. The double sign may be taken from the expression by recollecting that in the case of ϕz^{-1} or $\phi_0 z^{-1}$ it is *minus*. The formula in its simplest and definite state is therefore

$$\phi_n z + (-1)^{n+1} \phi_n z^{-1} = u_n (c + \Phi). \qquad (9)$$

We have now only to define the constants $u_0 c.\ u_1 c_1 \ldots u_n c$. This it is plain may be accomplished at once by taking z of such a value x, that Φ or $\int \psi z\, dz = 0$; for we then have in general

$$\phi_n x + (-1)^{n+1} \phi_n x^{-1} = u_n c. \qquad (10)$$

from which $u_n c$ may be determined, or at least may be expressed in terms of $u_1 c$. The method of this reduction will lose much of its intricacy and little of its generality, if at this stage of the process we somewhat particularize the functions. We may in conformity with equation (8) suppose

$$\psi z\, dz = \frac{dz}{z}$$

and the supposition will give us Φ an integrable expression, for

$$\int \psi z\, dz = \Phi = \log z.$$

Now $\log z$ becomes 0 when $z = 1$; we therefore derive from equation 10,

$$\begin{array}{ll} u_0 c = 0 & u_1 c = 2\phi_1 1 \\ u_2 c = 0 & u_3 c = 2\phi_3 1 \\ u_4 c = 0 & u_5 c = 2\phi_5 1 \\ & \\ u_{2m} c = 0 & u_{2m+1} c = 2\phi_{2m+1} 1 \end{array}$$

It only remains for us to determine $2\phi_{2m+1} .1$ in terms of $2\phi_1 1$.

This

This may easily be accomplished by many means when the character of $\varphi_n z$ is slightly known. In pointing out one means I take advantage of the labours of Spence. We can state the expanded expression

$$\varphi_{2m+1} z + \varphi_{2m+1} z^{-1} = 2\varphi_{2m+1} . 1 + 2\varphi_{2m-1} . 1 . \frac{\log z^2}{1.2}$$

$$+ 2\varphi_{2m-2} . 1 . \frac{\log z^4}{1.2.3.4} + \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$+ 2\varphi_1 . 1 . \frac{\log z^{2m}}{[2m]^{2m-1}} \ldots$$

And if z be made $= -1$, and a substitution made for $\log -1$ in terms of John Benouilli's quadrature of the circle, which might be deduced from the imaginary expressions for the sine and cosine, we shall have a particular series in terms of $\varphi_n . 1$, $\varphi_n - 1$ and π. If again $\varphi_n z$ be supposed to consist either of odd powers or of even powers of z (which it may be seen from the constitution of Φ it will do, if $\varphi_0 z$ consists of even or odd powers), then will be given a very simple relation betwixt $\varphi_n 1$ and $\varphi_n - 1$. Let it consist of odd powers, and we shall then have

$$2\varphi_{2m+1} 1 = \varphi_{2m-1} 1 . \frac{\pi^2}{1.2} - \varphi_{2m-3} 1 . \frac{\pi^4}{1.2.3.4}$$

$$+ \varphi_{2m-5} 1 . \frac{\pi^6}{1.2.3.4.5.6} - \ldots\ldots\ldots \pm \varphi_1 1 . \frac{\pi^{2m}}{[2m]^{2m-1}}$$

whence we may calculate

$$\varphi_3 1 = \frac{\pi^2}{1.2} . \frac{\varphi_1 1}{2}$$

$$\varphi_5 1 = \frac{\pi^4}{1.2.3.4} . \frac{\varphi_1 1}{1}$$

$$\varphi_7 1 = \frac{\pi^6}{1.2.3.4.5.6} . \frac{17 \varphi_1 1}{4}$$

$$\varphi_9 1 = \frac{\pi^8}{1.2.3.4.5.6.7.8} . 31 . \varphi_1 1.$$

&c. &c.

I shall afterwards have occasion to give a simple and commodious formula for the general determination of $\varphi_n 1$ in such series, in terms of $\varphi_1 1$; so that I am saved the necessity of digressing at this period of the paper. It is sufficient in the mean time to see that the constants may be determined, and of course that the quantity

$$u_n (c + \Phi) \qquad \text{is } \textit{given}.$$

Let us now seek the illustration of a particular case. φz will

will satisfy all the conditions which restrict it, if we suppose it equal to

$$\frac{1}{z+z^{-1}} \text{ or } \frac{z}{z^2+1}.$$

Whence
$$\varphi z = z - z^3 + z^5 - z^7 + \&c.$$

$$\varphi_1 z = \int \frac{dz}{z} \varphi_0 z = z - \frac{z^3}{3} + \frac{z^5}{5} - \frac{z^7}{7} + \&c.$$

$$\varphi_2 z = z - \frac{z^3}{3^2} + \frac{z^5}{5^2} - \frac{z^7}{7^2} + \&c.$$

$$\cdots\cdots\cdots\cdots\cdots\cdots$$

$$\varphi_n z = z - \frac{z^3}{3^n} + \frac{z^5}{5^n} - \frac{z^7}{7^n} + \&c. \qquad (11)$$

It might be seen from a foregoing part of the paper that $\varphi_1 1$, or

$$1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \&c.$$

is equivalent to $\frac{\pi}{4}$ when radius 1....The right hand member of equation (9) is thus referred to circular arcs, and logarithms of z. Let us now separate the cases in which n is an even or odd number, and we have from (10) and (11)

$$u_{2m}(c + \Phi) = \varphi_{2m} z - \varphi_{2m} z^{-1} = (z - z^{-1}) - \frac{z^3 - z^{-3}}{3^{2m}}$$
$$+ \frac{z^5 - z^{-5}}{5^{2m}} - \&c.$$

$$u_{2m+1}(c + \Phi) = \varphi_{2m+1} z + \varphi_{2m+1} z^{-1} = (z + z^{-1}) - \frac{z^3 - z^{-3}}{3^{2m+1}}$$
$$+ \frac{z^5 + z^{-5}}{5^{2m+1}} - \&c.$$

which if $z = e^{x\sqrt{-1}}$ give us the series

$$\frac{1}{2\sqrt{-1}} \cdot u_{2m}(c + \Phi) = \sin x - \frac{\sin 3x}{3^{2m}} + \frac{\sin 5x}{5^{2m}} - \&c.$$

$$\frac{1}{2} \cdot u_{2m+1}(c + \Phi) = \cos x - \frac{\cos 3x}{3^{2m+1}} + \frac{\cos 5x}{5^{2m+1}} - \&c.$$

It is now time to conclude. It must not be conceived that all this analytical labour has been spent merely in the investigation of the foregoing two series;—interesting as they are, they might in this case be thought dearly obtained. I have attempted throughout merely to sketch the leading features of all similar investigations, and to leave untouched only such difficulties as an analyst of very moderate pretensions may grapple with. The field which I have sought to point out, has to be explored; and I am convinced that the contents of it

it would amply repay the labour. I must apologize to your readers for the mistakes which will be found in the investigation. I already see several which it is impossible now to correct. I have been forced to draw it up during moments of leisure snatched now and then from the toils of a laborious employment, and of a host of other less congenial though more necessary pursuits. Let this stand for my excuse. Detached and fragmentical as it is, I hope that my leading purpose is in some degree accomplished. The general inquiry, however, into the decomposition of B_z is much more interesting and more practically useful; and as soon as I can command sufficient time to commit to paper the substance of what exists in my imagination respecting it, I will lay it before you with my best ability.

April 15, 1825. Σ.

LVI. *On the Determination of the Latitude of a Place by Means of the Transit Instrument placed perpendicularly to the Meridian. By Professor* Bessel. *Communicated in a Letter to Professor* Schumacher, *dated Königsberg, Feb.* 2, 1824.*

WHEN, in the year 1819, I saw you at Lauenburg, and remarked to you that it might be advantageous to determine the differences of the altitude of the pole (for the purpose of measuring geographical degrees) by means of a transit instrument, moving nearly perpendicular to the meridian, I had in view the difficulties often experienced in observing zenith distances. This difficulty is certainly removed by Ramsden's zenith sector, used in the English admeasurement of degrees for observing the differences of latitude, as well as by yourself and M. Gauss in your great undertaking of the same kind. It is also removed by the employment of Reichenbach's meridian circle for the purpose of measuring the zenith distances of stars passing near the zenith; for it is with such stars that the instrument gives the correct results, without the investigation of other zenith distances. I do not wish to refuse the fullest confidence to *such* means for obtaining the differences of latitude: therefore any other proposal might at present appear superfluous.

But if we are desirous to obtain a certain degree of accuracy with much less trouble, or try the results already obtained by a different method, I have no doubt but that it might be effected by the method which I am about to propose. My intention is to avoid entirely the divisions of the circle, and

* From Schumacher's *Astronomische Nachrichten*, No. 49.

to deduce the results by means of the watch; which I effect in the following manner.

I place a transit instrument (whose axis is horizontal, and whose line of collimation is correctly adjusted) so that the middle wire should describe a vertical circle. Its axis should lie nearly in the meridian, so that the vertical circle should stand nearly east and west, and thus twice traverse the parallels of all the stars which culminate between the equator and zenith.

The observation of the two times T and T′ in which the star passes the wire of the telescope will give the altitude of the pole as well as the zenith distance of the star on the meridian; and by repeating this observation in another place we shall obtain the difference in the altitude of the pole, or zenith distance, almost independently of the assumed declination of the star. However, the times T and T′ must be taken from a watch or clock which shows sidereal time: but it is not necessary that the correction of the clock, for the purpose of reducing it to the sidereal time, need be known.

In order to show the peculiarity of this method in a general view, I shall not take into account, at first, that the vertical circle described by the instrument is directed exactly east and west, but point out the results of, and the errors which might arise from, some elements of calculation, independently of that supposition.

I denote the correction of the clock by τ and τ', taking them (as well as the observed times) in degrees, minutes and seconds; the right ascension and declination of the star by α and δ; and the polar height by φ. According to these designations the two hour angles of the star (negative if easterly) will be

$$t = T + \tau - \alpha \quad . \quad t' = T' + \tau' - \alpha$$

and the cotangent of the azimuth will be

$$= \frac{\cos t . \cos \delta . \sin \varphi - \sin \delta . \cos \varphi}{\cos \delta . \sin t} = \frac{\cos t' . \cos \delta . \sin \varphi - \sin \delta . \cos \varphi}{\cos \delta . \sin t'} .$$

If we eliminate the azimuth from these two equations, we obtain

$$\tan \varphi = \tan \delta \times \frac{\cos \left(\frac{t' + t}{2}\right)}{\cos \left(\frac{t' - t}{2}\right)}$$

or, by introducing the values of t and t',

$$\tan \varphi = \tan \delta \times \frac{\cos \left(\frac{T' + \tau' + T + \tau}{2} - \alpha\right)}{\cos\left(\frac{T' + \tau' - T - \tau}{2}\right)}$$

If we now assume that δ, α, τ', τ are incorrectly known, and require the corrections $d\delta$, $d\alpha$, $d\tau'$, $d\tau$, we obtain thereby the correction of φ deduced from the formula just given, as under:

$$d\varphi = d\delta \cdot \frac{\sin 2\varphi}{\sin 2\delta} + d\alpha \cdot \tfrac{1}{2}\sin 2\varphi \cdot \tan \tfrac{1}{2}(t' + t)$$

$$- d\tau \cdot \frac{\sin 2\varphi \cdot \sin t'}{2(\cos t' + \cos t)} - \delta\tau' \cdot \frac{\sin 2\varphi \cdot \sin t}{2(\cos t' + \cos t)}$$

Let us now suppose that the instrument is correct to about a minute, moving from east to west; it is evident that the cosine may be placed = 1, in the numerator of the result ($\tan\varphi$) and we shall obtain

$$\tan\varphi = \tan\delta \cdot \sec\tfrac{1}{2}(T' + \tau' - T - \tau)$$

$$d\varphi = d\delta \cdot \frac{\sin 2\varphi}{\sin 2\delta} + \tfrac{1}{4}(d\tau' - d\tau)\sin 2\varphi \cdot \tan\tfrac{1}{2}(T' + \tau' - T - \tau)$$

Whence it appears that an error in the difference of the correction of the time as shown by the clock (the influence of which is so much smaller, the smaller $T' - T$ is, in the case in which this method is to be applied alone, viz. when the star culminates near the zenith) may be considered as trifling. Such an error would arise from the supposed incorrectness of the clock; but we may suppose that this is generally much better known than may be required in this method.

There remains, therefore, only the error of declination, which is

$$d\varphi = d\delta \cdot \frac{\sin 2\varphi}{\sin 2\delta}$$

For a star passing through the zenith, the altitude of the pole has exactly the same error as that of declination. For a star culminating south of the zenith, the error is greater; at least in our latitudes. Suppose this method were to be applied to determine the difference of latitude between two places, such as 51° and 56°, and we were to select a star passing through the southernmost point of the zenith; the error in this point would be = $d\delta$; and in the northern place $= 1{\cdot}055\, d\delta$. And the error in the difference of the polar altitudes $= 0{\cdot}055\, d\delta$; or even for $d\delta = 2''$, only $0''{\cdot}11$. If both places were equidistant north and south from the 45th degree of latitude, the difference would be found strictly correct. Moreover, it is doubtful whether the absolute values of the divisions of the zenith sector can be so correctly determined, that it may not have, in an arch of 5°, a greater inaccuracy than $0''{\cdot}11$; at least I consider this as much more difficult than the determination of the declination of a star to $2''$.

This method is peculiarly recommendable on account of its independence of any error in the instrument. If the collimation should not be sufficiently corrected, the cylinders of the axis should be unequal in their diameter, the telescope or the axis should bend, &c., we shall still obtain a correct result, either by reversing the axis between the two operations, or by observing one day in one position and the next in the other position of the

the axis, and taking the mean of both. The success solely depends on the quality of the telescopes and the care employed in the *levelling* of the axis. It also appears to me that those astronomical amateurs who possess but indifferent instruments for the measuring of angles, would thus obtain a determination of their latitude with greater certainty by means of a small and portable transit-instrument, even if it were not more powerful than the common telescopes in the small repeating circles. This method, in its application for the determination of declinations under the supposition of the latitude being known, has the advantage of being quite independent of the refraction; but it can only be employed for stars north of the equator.

What ought to render this method still more interesting to *you* is, that your famous countryman *Olaus Römer* (who in his ideas of astronomical observations and instruments has surpassed many of our moderns) employed, 120 years ago, a transit instrument placed from east to west, which is described in Horrebow's works, vol. iii. pp. 228—240, and who assigns the imperfection of the instrument as the reason for its not having been subsequently employed for the observation of declinations.—The work just alluded to, which I obtained but a few weeks ago, contains so many excellent things of Römer, that I am inclined to consider it as one of the most important works on practical astronomy; and I take this opportunity to observe *how much* might have been done in the art of observing, even in Römer's time, if the path he took had not been again abandoned.

[*Note by the Editor.*—The work here alluded to by M. Bessel is entitled "Petri Horrebowii, Opera Mathematico-physica." Hauniæ, 1740, 3 vols. quarto. The third volume contains the following treatise: "Basis Astronomiæ, sive Astronomiæ pars mechanica, in qua describuntur observatoria, atque instrumenta astronomica Rœmeriana Danica; simulque eorundem Usus, sive Methodi observandi Rœmerianæ." The xviiith chapter of this treatise is entitled "De instrumento Æquinoctiorum Rœmeri:" and this is the instrument alluded to by M. Bessel.

But the use of a special instrument, for the purposes here alluded to, is now superseded by the introduction of the altitude and azimuth instrument; which seems peculiarly adapted for observations of this kind.

If the circle be placed *exactly* east and west, we shall have

$$\cot \varphi = \cot \delta \,.\, \cos \tfrac{1}{2}\,(T'-T)$$

where $(T'-T)$ denotes the *correct* interval of *sidereal* time elapsed between the two observations, expressed in *degrees*, &c. But, if $(T'-T)$ be taken in *mean solar* time, we must multiply it by 1·0027379 in order to reduce it to sidereal time.]

LVII. *A De-*

LVII. *A Description of a new Patent Instrument, or Celestial Compass, adapted for ascertaining the Deviation of the Magnetic Needle, by simple Inspection, in any Part of the World; for finding the Latitude when the Horizon is obscured; and for steering Ships without Magnetic Aid. Invented by* GEORGE GRAYDON, *Captain in the Corps of Royal Engineers.**

General Description of PLATE I.

PLATE I. represents the celestial compass mounted in gimbals, as a means of detecting by simple inspection, as long as any of the heavenly bodies are visible, the changes to which the magnetic needle is exposed in all parts of the world, in consequence of the local magnetism of the earth† or meteoric influence, independent of the local attraction of the iron of the ship, or the annual change of the variation of the needle. It serves also as a substitute for the magnetic compass in high northern or southern latitudes, where the directive power of the magnetic compass becomes almost useless from its feeble and uncertain action.

In many cases it is impracticable to calculate the variation, in consequence of the state of the sky or atmosphere precluding the observation for an amplitude or azimuth; the former can only be taken at the rising or setting, and the latter can only be taken with accuracy when the sun or star is at low altitudes. It will frequently happen, and particularly in the channels of the British isles, that the horizon, and several degrees above it, is so obscured for some days together, that these observations cannot be made, although the sky is sufficiently clear at a greater altitude. In these circumstances no variation can be calculated with accuracy for those days, except from the table of variation, which is now becoming every day more and more uncertain. It is also to be observed, that, often, the detection of those accidents above mentioned (indeed almost always) depends on the observations, and may remain imperceptibly operating for a length of time, during which no observations to correct the variation can be had.

Plate I. also represents the celestial compass adapted for ascertaining the latitude when the horizon is obscured, or is rendered uncertain by refraction.

Description of Plate I. *of the Celestial Compass as adapted for detecting by simple Inspection, as long as any one of the Heavenly Bodies is visible, the Deviation of the Magnetic Needle.* (*See* Plate I.)

A B represents the face or dial plate of the instrument; this plate

* The Celestial Compass may be procured at Messrs. Warre and Brothers, 13, Austin Friars.

† Instances of the powerful effect which the local magnetism of the earth has upon the magnetic needle, are given in Appendix No. II.

plate is screwed down upon the upper part of a box or case, C, of a hemispherical form. The hemisphere C is suspended upon pivots or axes at *c*, which work freely through holes formed in a metal ring D; and this ring is suspended upon pivots or axes at *d*, being situated at right angles with the pivots *c*, before mentioned. The pivots *d* are adapted to turn in holes, or sockets, formed in adjustable bearings at the upper part of the two standards, or supports, E E; and the feet of the standards are screwed down upon a plate of metal, F G, which is capable of revolving upon an axis in the centre thereof, affixed to the stationary platform, or board, H I.

The plate F G has the cardinal points marked upon it, and is divided into degrees near its outer edge, being provided with a vernier I, affixed to the platform before mentioned. K L represents a heavy plate of metal, which is suspended from the pivots or axes *c* by two brackets, or open arms, one only of which arms is seen in the figure at M, the other being obscured by the hemisphere C.

The plate K L is situated considerably below the centre of gravity, or axis upon which the hemisphere C and ring are suspended, and thus tends always to maintain the instrument in a plane parallel to the horizon.

The plate K L has an arm or limb rising up from it at K, the upper part of which is provided with a vernier *k*, adapted to read off the degrees upon a graduated arc, *g h*, engraved on the side of the hemisphere C.

The under surface of the plate K L is provided with two plane mirrors, or reflecting surfaces, *m m*, which are situated in a frame affixed perpendicular to the plate K L, but in such a position, that the reflecting surfaces form a salient angle with each other.

These mirrors *, by reflecting the horizon from two different parts, furnish the means of adjusting the instrument into a horizontal position when in use. For example: When any two parts of the horizon are reflected in the mirrors *m m*, if the instrument be moved until the images of these two parts appear upon the mirrors in one straight line, that line being at the same time parallel to the edges of the mirrors, will indicate that the plate K L, to which they are affixed, is horizontal †.

The pivots, or axes, upon which the hemisphere is suspended, project some distance through the ring D, and are furnished

* See fig. 7, Plate II.; also see the end of Appendix No. I.

† Another method of adjusting the instrument to a horizontal position, when the horizon is obscured, is given at the end of Appendix No. I.

with

with small screw caps, which may be screwed on or off, for the purpose of giving the hemisphere a slight motion endways or in the direction of its axis, in order to adjust it into a proper position to balance correctly or horizontally in its gimbals.

The hemisphere C contains a weight or counterpoise within it, so situated that the centre of gravity should fall as nearly as possible in the centre of the hemisphere; or the centre of gravity should coincide as nearly as possible with the axis of its motion upon its pivot *c*, so that the horizontal position of the plate K L would not be disturbed by any turning or change of position of the hemisphere C upon its axis. The adjustment of its equilibrium may be performed by screwing up or down the small spherical weight Z, which is tapped upon a wire projecting from the top of the frame or tablet P.

In making use of the instrument for the purpose of finding the deviation of the magnetic needle, the direct light of the sun is made to fall upon a pair of cross wires, or the sun's rays are concentrated upon the tablet P by means of a lens at O instead of the cross wires, so as to direct their shadow upon a piece of ivory, marked with cross lines upon its surface; and the coincidence of the shadow of the cross wires with the intersection of the lines upon the tablet will determine the required position.

The dial plate A B is divided into 24 hours, or 360 degrees, and is provided with an hour or index hand E, which is formed to a vernier at one of its extremities, to read off the degrees upon the dial plate.

P represents a small frame, or square, which is mounted upon a pillar on the hand E, and the axis of which is in the centre of, and perpendicular to, the dial plate. The frame P is adapted to receive a piece of ivory, *q*, having cross lines or wires, intersecting each other upon its surface.

The hour hand E has, near one of its extremities, a small pillar or tube *o* projecting up from it, and into this tube a small round rod *s* is adapted to slide, having a square frame O at its upper extremity, furnished with cross wires, as represented in the drawing, Plate I.

The round rod *s* is graduated, and numbered with tangents to the angles of elevation or depression, above and below the level of the intersection of the cross lines upon the surface *q*, and it is provided with a vernier scale, formed on the side of the pillar *o*, to read off the divisions on the round rod. It is also furnished with a clamping and adjusting screw, as shown in the figure, to move it up or down with a slow motion.

Method

Method of using the Instrument for ascertaining the Variation of the Needle by Inspection. (*See* Plate I.)

Suppose the platform H I to be screwed upon the binnacle, or other convenient part of the ship, in such a position that the line H I, which is drawn upon the platform, may coincide exactly in a longitudinal direction with a line immediately over or parallel with the vessel's keel:—now, if the hemisphere C be elevated upon its axes or pivots *c*, until the divisions upon the arc *g h* are brought to read against the vernier *k* at the degree of the latitude of the place, it will be evident that the dial plate A B will be situated parallel to the plane of the equator, or perpendicular to the earth's polar axis. In this situation, if the hand or index E be set to the apparent time, say three hours, or forty-five degrees from the meridian, as shown in the figure, and the rod *s* be elevated in the tube *o* to the division corresponding with the tangent of the sun's declination, the compass plate, F G, which has the instrument mounted upon it, is then to be turned round upon its axis until the shadow of the point of intersection of the cross wires O may fall upon the surface of the ivory *q*, so as that the said point of intersection may coincide with the intersection of the cross lines upon the surface *q*. The line A B or F G will then be in the plane of the true meridian, and the variation of the magnetic compass in the binnacle, or any other magnetic compass which is parallel to it, may be ascertained by inspection. In whatever course or direction the vessel may be proceeding, if the compass plate F G is turned upon its axis until the vernier I is brought to coincide with the point or degree corresponding with that course, the shadow of the intersection of the cross wires O should always coincide with the intersection of the lines upon the ivory *q*.

The vessel will then proceed in its course without deviation, the instrument serving as a constant and immediate check to the irregularities of the needle as long as any one of the heavenly bodies is visible.

Method of using the Instrument for steering without Magnetic Aid. (*See* Plate I.)

The dial plate A B is provided with watch-work, by which the hand E is moved round once in twenty-four hours.

Having fixed the platform H I over or parallel to the vessel's keel, and the hand E being set to the time, and the hemisphere C being set to the latitude, as above described, the compass plate F G, which has the instrument mounted upon it, is to be turned round upon its axis, until the zero of the

vernier I is brought to coincide with such point or degree upon the compass plate, as will correspond with the course desired to be steered. For example: in the drawing, the compass plate is represented as set proper for steering a course due north.

The person who steers the vessel has only to keep it in such a position that the shadow of the round rod *s*, or of the intersection of the cross wires O, may fall upon the surface *q*, so as to coincide with the perpendicular mark thereon. The line A B or F G will then remain in the plane of the true meridian, and the vessel will therefore proceed in a course due north. In like manner any other course may be steered, by setting the compass plate F G accordingly, and keeping the shadow of the rod *s* upon the perpendicular mark upon the surface of the ivory *.

This method of steering is particularly applicable to high northern and southern latitudes, as the heavenly bodies remain constantly above the horizon, as long as navigation is practicable in those parts of the world, where the sky is generally unclouded and clear overhead; which circumstance is stated in Capt. Lyon's late account of his unfortunate attempt to reach Repulse Bay, from which the following is an extract, viz.:

"Although the fogs in the Polar regions are so frequently mentioned in the course of recent narrations which have been published, I believe they are generally understood as resembling our English fogs; which is not, in fact, the case.—In the northern seas, these vapours rarely rise to above a hundred feet from the sea, and a sky of most provoking brilliancy is frequently seen overhead."—*Capt. Lyon's Voyage of Discovery,* page 43.

An instrument on the construction above mentioned, for steering without magnetic aid, was ordered by the Lords of the Admiralty, and sent out with H. M. S. Hecla, on the Polar expedition, in the year 1824.

For ascertaining the Latitude when the Horizon is obscured. (*See* Plate I.)

The platform H I being fixed over or parallel to the vessel's keel, as above described, the instrument is to be turned upon the axis or pivot of the compass plate F G, until *o* on the outer division is brought to coincide with *o* on the vernier I. The round rod *s* is to be set to the sun's declination as above described,

* When the surface of the ivory cannot be distinctly seen by the person who steers, owing to its position with regard to the sun, a piece of semi-transparent glass is used (in place of the ivory); the shadow is then seen behind the glass.

scribed, and the line F G is to be placed north and south, by means of the magnetic compass in the binnacle*. The hemisphere C is then to be inclined upon its axis *c*, and the hand E moved, until the shadow of the intersection of the cross wires O is brought to coincide with the intersection of the marks upon the surface of the tablet *q*; the vernier *k* will then indicate the latitude of the place, by means of the divisions upon the arc *g h*.†

In like manner, the apparent time may be obtained when the latitude is given, by setting the arc *g h* to the latitude, the rod *s* to the sun's declination; the hand E is then to be moved until the shadow of the intersection falls upon the cross lines upon the ivory *q*, as before described. The vernier at the end of the hand E will then indicate the apparent time, even when the horizon is obscured, and when an altitude cannot therefore be taken‡.

Appendix.—No. I.

The following is the principle upon which the celestial compass is constructed, viz.:

The apparent motion of the sun being caused by the uniform rotation of the earth upon its axis from west to east once in twenty-four hours, it will appear evident, that, if an arm be moved with an equable motion in a contrary direction to the apparent motion of the sun, and having its axis of motion in a position parallel to the earth's axis §, the arm would keep pace with the apparent motion of the sun, while it describes its horary angle: if, therefore, the time be given, and the arm be set to that time, and be directed towards the sun, the noon hour line would then be situated in the true meridian.

For example, see fig. 4 and 5, Plate II.

Let O P represent the arm A F G, a circle described about its axis P, and divided into twenty-four equal parts or hours. When the arm O P is set to the apparent time,—suppose three hours, or 45° from the meridian,—the instrument is then to be

* Of course the variation must be allowed for.

† When the apparent time is given, the latitude may be determined without using the magnetic compass, by setting the hand E to the time, the rod *s* to the sun's declination, and then inclining the hemisphere C until the sun's rays, passing through the lens at O, are concentrated upon the middle of the tablet *q*, as before mentioned.

‡ As the method of adjusting the instrument to a horizontal position by means of the reflecting mirrors *m m*, cannot be used when the horizon is obscured, another mode of adjustment, by means of the sun's declination, is given at the end of Appendix No. I.

§ The earth, seen from the sun, would appear but as a point, and the centre of motion of the arm may therefore be considered as coinciding with that of the earth.

turned so as to bring the arm in a line between the axis P and the sun S. The arm will then, by its motion, continue to perform the same horary angle as the sun appears to describe, and A P, being the noon hour line, will therefore always indicate the true meridian.

In like manner, the true meridian may be indicated by means of a star, by calculating its distance from the plane of the meridian, or its horary angle at the time of observation, and setting the arm accordingly.

As the relative position of the sun or a star, with regard to the plane of the meridian, may be determined as above, its declination may also be determined as follows:—(See fig. 5, Plate II.)

Let fig. 5, Plate II. represent the exact position of the sphere, having its axis P P elevated to the latitude of the place, and the arm O *o* set according to the apparent time,—suppose three hours, or 45° from the meridian,—and directed to the sun S.

The meridian distance of the sun S, or the angular distance between the plane of the arm O *o* in that position, and the noon hour line A P, will be equal to forty-five degrees; because the instrument, being an exact representation of the position of the earth, having the axis P parallel to the pole, and the noon hour line A P parallel to the plane of the meridian, the horary angle is the measure of the meridian distance of the object S. In like manner, a line E Q, which is perpendicular to the axis P P, or to the pole, will be parallel to the plane of the equator, with which it will continue to maintain the same relative position during the motion of the arm O *o* round its axis P P.

As the arm O *s o* is an arc of a circle of which P is the centre, therefore, when the object has no declination, it will appear in the direction E Q, or the line S O P will coincide with the line E P Q. But when the object S declines from the equinoctial, the measure of its declination will be shown by the part of the arc O *s o* which is above E Q, of which arc the sight at P is the centre. The curve O *s* having a sight at O, is moveable within the fixed part of the arm *o*, and may be raised or lowered so as to bring the sight at O in a line between the object S and the other sight at P.

The fixed part of the arm at *o* is graduated from each side of *o*; the vernier upon the moveable curve at *o* will indicate the amount of the angle of elevation or depression of the sight O, above or below E Q, which angle is equal to the apparent declination of the object S, at the time of observation. For example, in the figure, the sight O is ten degrees above the equinoctial

noctial line E Q, because the vernier placed upon the moveable curve is ten degrees higher than *o* on the index of the fixed part of the arm. On the contrary, if the sight O were below the equinoctial line E Q, the vernier would be depressed in the same proportion below *o* on the index. The declination will of course be shown, not only for noon and midnight, but for any intermediate time, and therefore the quantity of increase or decrease for any given time will also be shown.

Method of Adjusting the Instrument to a Horizontal Position, by means of the Declination of the Object. (*See* Fig. 5, Pl. II.)

When the instrument is set in its position by means of the sun or a star, either of these objects itself will indicate when the instrument is truly horizontal: for this purpose the arm being previously set to the angle of the declination of the sun or star, and also to the apparent time, if the sight O is then found to be in a line between the object S and the other sight at P, it will be evident the bottom of the instrument or socket M will be parallel to the plane of the horizon.

Appendix.—No. II.

Extract from Capt. Parry's Voyage of Discovery in the Year 1823.

" The information obtained by Captain Lyon, on his late journey with the Esquimaux, served very strongly to confirm all that had before been understood from these people respecting the existence of the desired passage to the westward, in this neighbourhood, though the impossibility of Captain Lyon's proceeding further in that direction, combined with our imperfect knowledge of the language, still left us in some doubt as to the exact position of the strait in question: it was certain, however, that it lay somewhere in that direction to which we had been already so long and so anxiously looking, and that its eastern entrance was still occupied by many miles of fixed and therefore impenetrable ice; but the very impediment that had arrested Captain Lyon's progress, as well as our daily observations on the state of the ice near its outer margin, appeared to offer a considerable hope that this obstacle must in the course of nature very soon disappear, even by the gradual process of dissolution, if it were not more speedily removed by one grand and total disruption.

" While, therefore, Captain Lyon was acquainting me with his late proceedings, we shaped a course for Igloolik, in order to continue our look-out upon the ice, and made the tents very accurately by the compass, after a run of five leagues, when the Hecla hauled in shore to pick up one of her men that had been left

left there to procure game, and the Fury stood towards the margin of the ice. Just before we reached off the floe, the weather continuing extremely thick, with hard rain, I desired Mr. Crozier to set the extremes of the loom hanging over Igloolik, which was then on our lee quarter. He did so; but presently afterwards remarked that the compasses (both Walker's azimuth, and Alexander's steering) indicated the ship's head to be S.W. which was about the middle point on which, but a few minutes before, he had set the loom of the land two or three points abaft the beam. Knowing, by the true direction in which we were sailing, that the ship's course by the compass, if unaffected by any foreign local attraction, should have been about east, which in fact the needles had indicated previous to the change remarked by Mr. Crozier, I tried what tapping with the hand (the usual expedient in cases of mere sluggishness) would do, but without producing any effect. Being now obliged to tack for the ice, we carefully watched the compasses in standing off, and having sailed about a quarter of a mile, observed them both return gradually to their correct position. Being thus satisfied that some extraordinary local attraction was influencing the needles, we again tacked to repeat the experiment, and with a nearly similar result. The observations were then continued on one or two successive tacks, the ship being steadily steered upon a given point, by some object a-head; and an account of the whole is subjoined in one connected view. The observations were made between six and nine P.M. The wind being moderate at east (true), the weather very rainy, the soundings fifty-two fathoms, and the nearest land distant from six to eight miles.

"The space sailed over during the time the changes were taking place, did not exceed a quarter of a mile.

"Starboard tack, compasses first indicating the ship's head East, then changed to South-west.

Larboard ........................N.W. b. N.................S.W. ½ W.
Starboard .... East........................S.S.E.

Bore aware to endeavour to cross our original track.

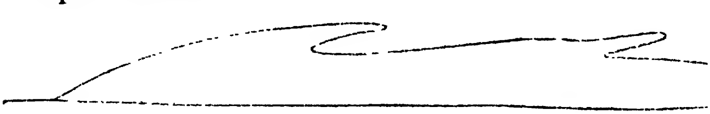
Larboard tack { Alexander's compass N.W. b. N...W. b. S.
Walker's .... N.W........ W.S.W.
Starboard, both compasses............... East.........S.W. ½ S.
Larboard { Alexander's ... N.W. ¾ N...... S.W. b. W. ¾ W.
Walker's...... N.W. .... S.W. b. W. ¼ W.
Starboard tack, both compasses .... N.E. b. E. ¾ E. ... E. ¾ E.

Alexander's, a minute or two after, returned to N.E. b. E. ¾ E. and Walker's to E. ½ N. Alexander's compass was placed on the binnacle, the other stood about five feet higher, in its usual place.

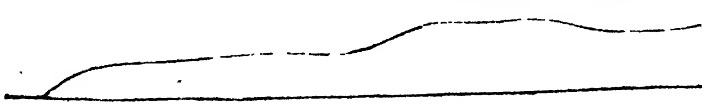
"In

"In order to follow up the observations on this phenomenon on some other day, I sent a boat to fix a flag upon the ice, by way of marking the spot, but the margin was so broken up that it was impracticable to land upon it; a light buoy was therefore moored for the same purpose, though with little chance of retaining its station, on account of the depth of water. During the remainder of the night, when the wind and weather obliged us to keep more to the northward, the compasses were not thus influenced *.

"The weather cleared up in the morning of the 2d. We found that a strip of ice about half a mile in width had been lately separated from the fixed ice, but this to our impatience appeared like a drop of water in the ocean; considerable streams and patches were also drifting along the margin during the day, and we were employed in breaking through them in order to make fast to the floe, the weather being unfavourable for keeping under way. In the evening we secured the ships to the ice, being in twenty-three fathoms, at the distance of two miles to the westward of Tern Island. For several hours in the course of this day there was something in the atmosphere which distorted objects into very curious shapes. The principal feature in this phenomenon was a constant waving tremulous motion near the horizon, causing the whole body of ice to appear at times as if turning round, and making one almost giddy to look steadfastly at it. The distant land was sometimes flattened down so as to appear like a single thick black line upon the horizon; then again it would assume a shape of this kind—



when its real outline, when not thus distorted, was this,—



"The tremulous appearance is, in a greater or less degree, a very common phenomenon in the Polar seas. Such, indeed, is the frequent occurrence of extraordinary and variable terrestrial refraction, and the consequent uncertainty with respect to the dip of the horizon, that observations made by the hori-

* The spots near which this local attraction was found, are designated on the chart by this mark, ⊕.

zon

zon of the sea, even when wholly free from ice, cannot be depended on within two or three minutes. There is, however, practically, little or nothing to regret on this account, from the almost constant opportunities that occur in these seas of resorting to the more accurate method of observation by artificial horizons. The weather, which had for several hours been rainy and thick, cleared up about noon on the 4th. In consequence of the wind shifting to the N. W. we made sail from the floe, in order to look for the buoy and to continue our observations on the magnetic attraction in that neighbourhood. After making several tacks as near the place as the bearings of the land and the soundings could direct us, but without discovering the buoy, we were obliged for the present to give up the attempt; having to our great satisfaction observed a floe, at least three miles in length and two in breadth, just detached from the fixed ice, and rendering it necessary for us to work out of its way lest it should force us towards the shore: we only, therefore, waited to put down some nets to ascertain the nature of the bottom, and then hauled round the floe. A quantity of shells, among which were a few of the new species of Anomia discovered on the last voyage, with some shrimps and echini, were all that we could thus fish up. Having cleared the end of the floe, which drifted rapidly away, and, as usual here, never made its appearance afterwards, we made the ships fast to the fixed ice P. M. having by the late disruption made considerable progress in the direction of the strait."

Page 317.—"The wind gradually falling, was succeeded by a light easterly breeze, with which, at daylight on the 26th, we steered under all possible sail up the strait. The course being shaped, and no ice in our way, I then went to bed; but was immediately after informed by Mr. Crozier that the compasses had shifted from N ½ E. (which was the course I left them indicating) to E ½ N., being a change of seven points in less than ten minutes. After running half a mile in a true W. by N. direction, the needles began to return to their true position; in half a mile farther they had resumed their proper direction, and agreed exactly at North. Having sent a boat to the Hecla immediately on our noticing the first alteration, I found from Captain Lyon that a similar phenomenon was observed to take place on board that ship, which was following in our wake. The breeze slowly increasing from the eastward, and the weather happily remaining unusually clear for that direction of the wind, we soon arrived off the narrow part of the strait; immediately on opening which, we met a tide or current running above two knots to the eastward, with nume-

rous eddies and ripplings. By keeping on the South, or continental shore, and passing along by Cape N.E. within two or three hundred yards of the rocks, we succeeded, with the assistance of the boats a-head, in getting through the channel soon after eleven o'clock."

Extract from Captain Lyon's Voyage of Discovery.

Latitude 61° 13′— Longitude 63° 54′.

"The deviation of our compasses was here very great and irregular, although less so with our head Northward, than otherwise: even Gilbert's excellent azimuth compass required constant tapping, although under the influence of Professor Barlow's plate, which had hitherto corrected it with the greatest accuracy."—Page 30.

"The stillness of this day was highly favourable for obtaining observations for the dip of the needle; but the floe to which we were fast was not of sufficient extent to admit of our getting so far from the ship as to be free from her attraction: I was now the more desirous of obtaining these observations, on account of the fast increasing sluggishness of the compasses,—for that of Gilbert's, which had hitherto been fully corrected for the local attraction of the ship by Barlow's plate, now began to show nearly as much deviation when our head was to the Eastward, as any of the other compasses."—Page 44.

"Our compasses had now become quite useless, with our head Southerly; and that in particular to which the plate was fitted, so powerless, that its North point stood wherever it was placed by the finger; but with our head Northerly, they all traversed again. This, however, benefited us but little; for, as our route lay to the South-west, we were without other guidance than celestial bearings, which could not always be obtained."—Page 53.

"In the forenoon watch, our larboard compass, which, with two others, had shown our head N. b. W. which (with three points and a half westerly variation) agreed with the sun's bearing, in giving a N. W. ½ W. course, suddenly pointed E.N.E., and no tapping or motion would keep it in any other point for two or three minutes; after which, it as suddenly recovered its agreement with the others, and continued quite correct. We now, from repeated observations, discovered that when our head was nearly North by compass, the deviation was three points and a half West; but when between North and West, it amounted to eight points; while with the head to the Southward, the compasses would generally rest wherever they were directed by the finger; and sometimes each persisted in maintaining a direction of its own.

"Barlow's plate now became useless, and its want of effect was decided by finding Gilbert's compass, while under its immediate influence, the dullest in the ship."

Ellis, in his account of the Expedition of the Dobbs and California, in 1746, says:—

"I cannot help taking notice in this place (while off Chesterfield Inlet), while amongst these islands, and in sailing through the ice, the needles of our compasses lost their magnetical qualities: one seeming to act from this direction, and another under that; and yet they were not for any considerable time constant to any. We laboured to remedy this evil, by touching them with an artificial magnet, but to very little purpose; for if they recovered their powers by this means, they presently lost them again."—*Ellis's Account*, page 220, *in the Year* 1748.

Appendix.—No. III.

Extract from Capt. Franklin's Journey to the Polar Sea.—1819.

"*August* 12th.—Azimuths were obtained this evening that gave the variation 58° 45′ 0″, which is greater than is laid down in the Chart, or than the officers of the Hudson's Bay ships have been accustomed to allow. Latitude 57° North." —Page 26.

"*August* 19th.—Nothing worthy of remark occurred, except the rapid decrease in the variation of the magnetic needle.

"N. B. At York factory the variation is only 6° 00′ 21″ East."—Page 32.

"*September* 25th.—About half a mile from the bend or knee of the lake, there is a small rocky islet, composed of magnetic iron ore, which affects the magnetic needle at a considerable distance. Having received previous information respecting this circumstance, we watched our compasses carefully, and perceived that they were affected at the distance of three hundred yards, both on the approach to and departure from the rock; on decreasing the distance, they became more and more unsteady; and on landing they were rendered quite useless, and it was evident that the general magnetic influence was totally overpowered by the local attraction of the ore. When Kater's compass was held near to the ground on the North-west side of the island, the needle dipped so much that the card could not be made to traverse by any adjustment of the hand; but on moving the same compass about thirty yards to the West part of the islet, the needle became horizontal, traversed freely, and pointed to the magnetic North. The dipping needle being landed on the South-west point of the islet, was adjusted as nearly as possible on the magnetic meridian,

by

by the sun's bearings, and found to vibrate freely: when the face of the instrument was directed to the East or West, the mean dip it gave was 80° 37′ 50″; when the instrument was removed from the North-west to the South-east point, about twenty yards distant, and placed on the meridian, the needle ceased to traverse, but remained steady at an angle of 60°; on changing the face of the instrument so as to give it a South-east and North-west direction, it hung vertically. The position of the slaty strata of the magnetic ore is also vertical,—their direction is extremely irregular, being much contorted." —Page 56.

"The observations of this evening seem to corroborate the remark which I had previously made, that the direction in which the needle moves seems to depend upon the position in which the streams of the aurora borealis are placed, and the quantity of its effect to its proximity to or distance from the earth. When the extremities of the arches lay near the bearings of 324° and 54°, the needle moved Eastward; and when near the bearing, 324° and 144°, or 279° and 99°, the motion of the needle was Westward: both of these facts were shown to-night. At the first display, when the extremities of the arches pointed near 324° and 54°, and the interior motion followed the same direction, the needle moved Eastward as far as 345°, but after midnight, coruscations ceased to appear in that direction, and at 12^h 10^m, were presented in three arches traversing the zenith, whose extremities pointed 121° and 302°; the needle then receded towards the West, and rested at 349° 30′, having varied its position 5° 40′ in the course of twenty minutes."—Page 560.

"I apprehend much of the irregularity in the result of the observations for the variation of the compass along the Copper Mine River is to be attributed to local causes of attraction, and particularly to the existence of iron ore among the rocks, which is very general: a greater intermixture of iron ore was perceived in the rocks on the sea coast, than on the banks of the Copper Mine River."—Page 635.

"The seat of the phenomenon of the aurora borealis lies between the latitudes 64° and 65° North, or about the position of Fort Enterprize, because the coruscations were as often seen there in the southern as in the northern parts of the sky."—Page 553.

The Island of Canna, on the western coast of Scotland, affords a remarkable instance of the effects on the magnetic needle from the local attraction of mountains (charged with iron ore).—*See Murray's Tour to the Hebrides.*

LVIII. *A Binary*

LVIII. *A Binary Arrangement of the Class* Amphibia. *By* A. H. HAWORTH, *Esq., Fellow of the Linnæan and Horticultural Societies of London, and of the Imperial Natural History Society of Moscow, &c. &c.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

IN order that some other class of Natural History might be subjected to the test of my *binary* method of arrangement besides that of *Crustacea*, I send you hereunder, a table, so disposing of all the numerous genera of the Class *Amphibia*, or Reptiles; as far at least as they are published in the elaborate work of Merrem, on that class, in 1820; and who is at once the best and last author on this subject. And to those I have added the three most extraordinary and colossal fossil genera *Ichthyosaurus*, *Plesiosaurus*, and *Megalosaurus*, which have latterly so extensively interested both the geological and zoological world.

In my last communication I noticed to you the manner in which the analogies and affinities of Natural History are indicated in my tables, and how these insensibly blend into each other. And I may now further observe, that every dichotomy of the tables, if viewed and taken with the root (or semi-dichotomy) from which it immediately proceeds, may be considered as a sort of triangular circle returning affinitatively into itself: and that the two further dichotomies issuing from each of its branches may be also considered as forming with it a still larger and broader-based triangular or pyramidal circle (inclosing the former): and so, onwards, until we arrive at the Genera; and including finally (circle within circle) every group in the table of Nature; together with all the various *genera* into which each group is capable of being divided; and into which each respectively extends, and ends: the great whole composing the vast homogeneo-heterogeneous circle of Nature— "Ubique varians semper tamen eadem."

I remain, sir, yours, &c.

Queen's Elm, Chelsea, April 22, 1825. A. H. HAWORTH.

P. S. The generic names are ever in *italics* in the tables, to distinguish them to the current reader promptly from all others; showing at the same time their due locations, as they occur in the continuous way of a straight line.

AMPHI-

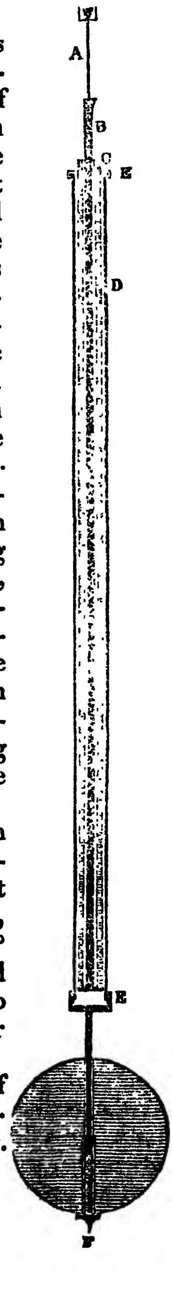
AMPHIBIORUM CONSPECTUS DICHOTOMUS.

Amphibia.

- PHOLIDOTA.
  - *FORNICATA.*
    - EDIGITATA.—*Caretta, Sphargis.*
    - DIGITATA.—*Testudo, Matamata, Emys, Tetrapene, Chersine.*
  - *EFORNICATA.*
    - LORICATA
      - Marina ?—*Ichthyosaurus, Plesiosaurus, Megalosaurus.*
      - Crocodilia.—*Alligator, Campsa, Gavialis.*
    - SQUAMATA.
      - Pedata.
        - Scansoria.—*Chamæleon....*
        - Gradientia.
          - Tetrapoda
            - Communipedes.
              - Inextensilingues.—*Gekko, Anolis, Basiliscus, Draco, Iguana, Polychrus, Pacuster, Lyriocephalus, Calotes, Uromastyx, Zonurus.*
              - Extensilingues.—*Uranus, Teius, Lacerta, Tachydromus.*
            - Brevipedes.—*Scincus, Gymnopthalmus, Tetradactylus, Colchicus, Monodactylus.*
          - Dipoda.
            - Dactylati.—*Bipes, Pygodactylus.*
            - Adactyli.—*Pygopus, Pseudopus.*
      - Apoda.
        - Palpebrata.—*Hyalinus, Anguis, Acontias.*
        - Epalpebrata.
          - Gulonia.
            - Innocua.—*Acroderus, Rhinopirus, Torquatrix*, Eryx, Boa, Python, Scytale, Coluber, Dryinus †.*
            - Venenata.
              - Solididentes.—*Bungarus, Trimeresurus, Hydrus.*
              - Insolididentes.—*Platurus, Elaps, Ophryas, Naia, Pelias. Vipera, Cophias, Crotalus, Langaha.*
          - Typhlinia.—*Typhlops, Amphisbæna.*
- BATRACHIA.
  - *APODA.*—*Cæcilia...*
  - *PEDATA.*
    - SALIENTIA.—*Pipa, Calamita, Bufo, Bombinator, Breviceps, Rana.*
    - GRADIENTIA.
      - Mutabilia.—*Salamandra, Molge.*
      - Immutabilia.—*Hypocthon, Siren.*

* *Tortrix* Merrem, Lepidopterorum nomen, mutavi ad *Torquatricem.*
† In Entomologiâ etiam nomen.

LIX. *On a new Compensation Pendulum. By* WILLIAM HERAPATH, *Esq.*



IT has long been an object with astronomers and navigators to possess a cheap and correct pendulum, which shall have the power of correcting its expansion or contraction from heat and cold. The principle adopted in the construction of such pendulums has been, that of an expansion upwards by one metal just equal in amount to the downward expansion of the pendulum rod. The most approved instruments of this sort are, what has been termed the Gridiron pendulum, Mons. Thiout's, and the mercurial one. These are so well known to scientific persons, that it is unnecessary to describe them here, but merely remark that neither of them have been extensively introduced. I conceive this non-introduction to arise, from the complexity of the gridiron pendulum, from the difficulty of making heat and cold act equally on the two rods of Mons. Thiout's (the one being outside the clock-case while the other is inside), and from the expense of the mercurial pendulum. Considering it possible to make a compact pendulum rod which shall not possess the disadvantages of the others, and that such an one would still be a desideratum, I shall proceed at once to the description, after having given the data upon which the calculations are founded.

The average expansion of iron, as taken from the experiments of Lavoisier and Laplace, Borda, Smeaton, Troughton, and Dulong and Petit between the temperatures of 32° and 212° F., for every 1·000000000 is ·00124869, or for 1° Fahr. is ·000006937. The expansion of steel spring for 1° Fahr. is ·00000761 according to Muschenbroek; and that of hammered zinc for 1° Fahr. is ·00001672 according to Smeaton.

If the pendulum be constructed as usual of three inches of watch spring, and the remainder of iron wire, the expansion for 1° of Fahr. will be

	3 inches steel	·000022830
	36·139290 iron	·000250600
		·000273430 in.

which would be counteracted by 16·35 inches of zinc. But as this cannot be applied without adding more iron, the zinc must be increased by so much as will also counteract that addition; and yet the zinc must not be greater or less in length than the iron added. The exact quantity I make to be 27·92 inches.

The expansion of pendulum rod will then be for 1° Fahr.

3 in.	steel	·000022830	
36·139290	iron	·000250600	
27·92	iron	·000193542	
67·059290 in.		·000466972	of 27·92 inches zinc will be ·000466822.

Having determined the length of zinc necessary, I apply it thus:—The pendulum rod is made as usual in common clocks, with three inches of steel spring attached to an iron wire, having a foot firmly fastened to it;—a tube of zinc 27·92 inches long, is slid over this rod and fastened to the foot: an iron tube is now put over this zinc tube, and at the top is fixed by a screw to the top of the zinc; the bottom of the iron tube is connected with the pendulum ball.—The annexed view will convey a better idea of the nature of the instrument than any verbal description.

A is the watch spring.
B the iron wire.
C the zinc tube.
D the external iron tube.
E E the screw collars for regulating the zinc tube.
F the screw for shortening the whole rod.

LX. *On the Gold Mines of North Carolina. By* Denison Olmsted, *Professor of Chemistry and Mineralogy in the University of North Carolina* *.

THE gold mines of North Carolina, which have recently become an object of much inquiry both at home and abroad, are situated between the 35th and 36th degrees of N. latitude, and between the 80th and 81st degrees of W. longitude from London. They are on the southern side of the State, not far from the borders of South Carolina, and somewhat westward of the centre. Through the gold country flows the river Pedee, receiving within the same district, the Uwharre from the north, and Rocky River from the south, both considerable streams. Above the junction with the Uwharre, the Pedee bears the name of Yadkin.

The gold country is spread over a space of not less than

* From the American Journal of Science and Arts: vol. ix.

1000 square miles. With a map of N. Carolina one may easily trace its boundaries, so far as they have been hitherto observed. From a point taken eight miles west by south of the mouth of the Uwharre, with a radius of eighteen miles, describe a circle,—it will include the greatest part of the county of Montgomery, the northern part of Anson, the north-eastern corner of Mulenberg, Cabarrus, a little beyond Concord on the west, and a corner of Rowan and of Randolph. In almost any part of this region, gold may be found in greater or less abundance, at or near the surface of the ground. Its true bed, however, is a thin stratum of gravel inclosed in a dense mud, usually of a pale blue, but sometimes of a yellow colour. On ground that is elevated and exposed to be washed by rains, this stratum frequently appears at the surface; and in low grounds, where the alluvial earth has been accumulated by the same agent, it is found to the depth of eight feet: where no cause operates to alter its original depth, it lies about three feet below the surface. Rocky River and its small tributaries which cut through this stratum, have hitherto proved the most fruitful localities of the precious metal.

The prevailing rock in the gold country is argillite. This belongs to an extensive formation of the same, which crosses the State in numerous beds, forming a zone more than twenty miles in width, and embracing, among many less important, varieties of slate, several extensive beds of novaculite, or whetstone slate, and also beds of petrosiliceous porphyry and of greenstone. These last lie over the argillite, either in detached blocks, or in strata that are inclined at a lower angle than that. This ample field of slate I had supposed to be the peculiar repository of the gold; but a personal examination discovered that the precious metal, embosomed in the same peculiar stratum of mud and gravel, extends beyond the slate on the west, spreading, in the vicinity of Concord, over a region of granite and gneiss.

A geographical description of the gold country would present little that is interesting. The soil is generally barren, and the inhabitants are mostly poor and ignorant. The traveller passes the day without meeting with a single striking or beautiful object, either of nature or of art, to vary the tiresome monotony of forests and sandhills, and ridges of gravelly quartz. Here and there a log hut or cabin, surrounded by a few acres of corn and cotton, marks the little improvement which has been made by man, in a region singularly endowed by nature. The road is generally conducted along the ridges, which slope on either hand into valleys of moderate depth, consisting chiefly of fragments of quartz, either strewed coarsely over the ground, or

or so comminuted as to form gravel; these ridges have an appearance of great natural sterility, which, moreover, is greatly aggravated by the ruinous practice of frequently burning over the forests, so as to consume all the leaves and under-growth, giving to the forest the aspect of an artificial grove.

The principal mines are three—the Anson mine, Reed's mine, and Parker's mine.

The Anson mine is situated in the county of the same name, on the waters of Richardson's creek, a branch of Rocky River. This locality was discovered only two years since by a "gold hunter,"—one of an order of people, that begin already to be accounted a distinct race. A rivulet winds from north to south between two gently sloping hills that emerge towards the south. The bed of the stream, entirely covered with gravel, is left almost naked during the dry season, which period is usually selected by the miners for their operations. On digging from three to six feet into this bed, the workman comes to that peculiar stratum of gravel and tenacious blue clay, which is at once recognised as the repository of the gold. The stream itself usually gives the first indication of the richness of the bed through which it passes, by disclosing large pieces of the precious metal shining among its pebbles and sands—such was the first hint afforded to the discoverer of the Anson mine. Unusually large pieces were found by those who first examined the place, and the highest hopes were inspired. On inquiry it was ascertained that part of the land was not held by a good title, and parcels of it were immediately *entered**; but it has since been a subject of constant litigation, which has retarded the working of the mine.

Reed's mine in Cabarrus is the one which was first wrought; and at this place, indeed, were obtained the first specimens of gold that were found in the formation. A large piece was found in the bed of a small creek, which attracted attention by its lustre and specific gravity; but it was retained, for a long time after its discovery, in the hands of the proprietor, through ignorance whether it were gold or not. This mine occupies the bed of Meadow creek, (a branch of Rocky River,) and exhibits a level between two hillocks, which rise on either side of the creek, affording a space between from fifty to one hundred yards in breadth. This space has been nearly all dug over, and exhibits at present numerous small pits for the distance of one fourth of a mile on both sides of the

* A piece of land is said not to be *entered* when it remains the property of the public, without taxation. Any one is at liberty to enter on the State books whatever land he can find in this situation, the land being secured to him on his becoming responsible for the taxes.

the stream. The surface of the ground and the bed of the creek are occupied by quartz and by sharp angular rocks of the greenstone family. The first glance is sufficient to convince the spectator that the business of searching for gold is conducted under numerous disadvantages, without the least regard to system, and with very little aid from mechanical contrivances. The process is as follows. During the dry season, when the greatest part of the level above described is left bare, and the creek shrinks to a small rivulet, the workman selects a spot at random, and commences digging a pit with a spade and mattock. At first he penetrates through three or four feet of dark-coloured mud, full of stones in angular fragments. At this depth he meets with that peculiar stratum of gravel and clay, which he recognises as the matrix of the gold. If the mud be very dense and tenacious, he accounts it a good sign; and if stains or streaks of yellow occasionally appear on the blue mud, it is a fortunate symptom. Sometimes he penetrates through a stratum of the ferruginous oxide of manganese, in a rotten friable state. This he denominates "cinders," and regards it also as a favourable omen. Having arrived at the proper stratum, which is only a few inches thick, he removes it with a spade into the "cradle." This is a semi-cylinder laid on its side, (like a barrel bisected longitudinally and laid flat-wise,) and made to rock like a cradle on two parallel poles of wood. The cradle being half filled with the rubbish, water is then laded in, so as nearly to fill the vessel. The cradle is now set to rocking, the gravel being occasionally stirred with an iron rake, until the coarse stones are entirely freed from the blue mud,—a part of the process which is the more difficult, on account of the dense adhesive quality of the mud. By rocking the cradle rapidly, the water is thrown overboard, loaded with as much mud as it is capable of suspending. The coarser stones are then picked out by hand, more water is added, and the same process is repeated. On pouring out the water a second time, (which is done by inclining the cradle on one side,) a layer of coarse gravel appears on the top, which is scraped off by hand. At the close of each washing, a similar layer of gravel appears on the top, which appears more and more comminuted until it graduates into fine sand, covering the bottom of the cradle. At length this residuum is transferred to an iron dish, which is dipped horizontally into a pool of water, and subjected to a rotary motion. All the remaining earthy matter goes overboard, and nothing remains but a fine sand, chiefly ferruginous, and the particles of gold for which the whole labour has been performed. These are frequently no larger than a pin's head,

head, but vary in size from mere dust to pieces weighing one or two pennyweights. Large pieces, when they occur, are usually picked out at a previous stage of the process.

Large pieces of gold are found in this region, although their occurrence is somewhat rare. Masses weighing four, five, and six hundred pennyweights, are occasionally met with; and one mass was found that weighed, *in its crude state*, 28 lbs. avoirdupoise. This was dug up by a negro at Reed's mine, within a few inches of the surface of the ground. Marvellous stories are told respecting this rich mass;—as that it had been seen by gold-hunters at night, reflecting so brilliant a light, when they drew near to it, with torches, as to make them believe it was some supernatural appearance, and to deter them from further examination. But all stories of this kind, as I was assured by Mr. Reed the old proprietor, are mere fables. No unusual circumstances were connected with the discovery of this mass, except its being nearer the surface than common. It was melted down and cast into bars soon after its discovery. The spot where it was found has been since subjected to the severest scrutiny, but without any similar harvest. Another mass weighing 600 pennywts. was found on the surface of a ploughed field in the vicinity of the Yadkin, twenty miles or more north of Reed's mine. Specimens of great elegance, as I should infer from the descriptions of the miners, are occasionally found; but for want of mineralogists to reserve them for cabinets, they have always been thrown into the common stock and melted into bars. Mr. Reed found a mass of quartz, having a projecting point of gold of the size of a large pin's head. On breaking it open, a brilliant display of green and yellow colours was presented, which he described as exceedingly beautiful. The gold weighed 12 pennywts. The mineralogist may perhaps recognise in this description a congeries of fine crystals, but on that point the proprietor could not inform me. Although fragments of greenstone and of several argillaceous minerals occur among the gravel of the gold-stratum, yet, in the opinion of the miners, the precious metal is never found attached to any other mineral than quartz. Indeed it is rarely attached to any substance, but is commonly scattered promiscuously among the gravel. Its colour is generally yellow with a reddish tinge, though the surface is not unfrequently obscured by a partial incrustation of iron or manganese, or by adhering particles of sand. The masses are flattened and vesicular having angles rounded with evident marks of attrition. The rounded angles and vesicular structure lead to the opinion, which is very general, that the metal has undergone *fusion;* but any one who inspects the

 specimens

specimens narrowly, will be convinced that their worn and rounded appearance is owing to attrition, and that the cavities are produced by the indentation of sand and gravel, the exact impress of which may be observed, and particles of them may still frequently be seen imbedded. The gravel, moreover, which is separated by washing, bears evident marks of attrition, of *a limited duration*, sufficient to round its edges and angles, but not sufficient to destroy them: the fragments are not ovoidal like the pebbles of rivers, but are still flat, retaining their original form, except that their edges are dull and their angles blunted. In short, the whole appearance is such, as would naturally result from so soft a substance as virgin gold being knocked about among such stern associates as quartz and greenstone.

The appearance of fusion, supposed to be exhibited by the gold, has inspired the idea among the miners, that the small pieces which they obtain have been melted out from some *ore* that lies disguised somewhere in the vicinity. This idea has frequently made them the dupes of imposition. The mineral rod, charms, and other follies, have had their reign here; and the first is still held in some estimation. The common rocks and stones of the country have been tortured by a new race of alchymists, who have imagined them to be the ore of gold, veiling, under some disguise, the characters of the precious metal. A great degree of eagerness also pervades the country on the subject of the metals in general. The minerals thrown out in excavating pits in search of gold, consist chiefly of quartz, greenstone, and hornblende mixed with chlorite, and afford little that is interesting to the collector of specimens. Almost the only substance which I met with, that was worth preserving merely as a specimen, was *pyritous copper*. Of this I saw some elegant fragments. It occurs in a gangue of quartz, and resembles that found at Lane's mine at Huntington, Con. (Amer. Journal of Science, vol. i. p. 316.). A vein of it occurs in slaty clay, six miles east of Concord, in Cabarrus county. This ore had been subjected to numerous experiments, on account of the belief that it was the "ore of gold" above mentioned; and, although the experiments did not lead to the discovery of gold, yet a "German miner and mineralogist" had, it was said, detected platina in it. On searching into the evidence of so unexpected a result, I was informed that a white metal was produced from this ore, which was not lead, nor tin, nor silver, but answered perfectly to the description of platina, although, as they acknowledged, it was easily fused, and burned with a blue flame. I suspected it to be *metallic antimony*, but still could perceive no signs of

of that metal in the ore. I requested a minute account of the process.—The materials, namely, the ore, charcoal, borax, &c. were put into a crucible—emetic tartar, in considerable quantity, was added to make the ore "spew out" the metal. Ipecacuanha was afterwarts tried with the same view, but was not found to be strong enough "to make the ore vomit." After the account of the process, it was not difficult to account for the production of antimony, it being obviously derived from the emetic tartar.

At Concord near the western limit of the gold country the metal is found in small grains in the streets and gullies, after every rain; and the gullies frequently disclose the stratum of gravel and mud, well known as the repository of the gold. Washings on a more limited scale are conducted here. The clay is not so dense at this place as at Reed's mine, but more ferruginous and full of spangles of golden-coloured mica. This stratum rests on *gneiss:* those before described were over the slate formation.

Parker's mine is situated on a small stream four miles south of the river Yadkin. As in the instances already mentioned, excavations were numerous in the low grounds adjacent to the stream; but, at the time of my visit, the earth for washing (which was of a snuff colour) was transported from a ploughed field in the neighbourhood, that was elevated above fifty or sixty feet above the stream. The earth at this place which contained the gold was of a deeper red than that at either of the other mines. The gold found here is chiefly in flakes and grains. Occasionally, however, pieces are met with which weigh 100 pennywts. and upwards; and very recently a mass has been discovered that weighed four pounds and eleven ounces. This is said to have been found at the depth of ten feet, which is a lower level than any I had heard of before. The idea of an aqueous deposit, which is apt to be impressed upon us whenever we either inspect the formation or reflect upon its origin, would lead us to expect, on account of the great specific gravity of gold, that the largest masses would be found at the lowest depths. But I am not aware that any uniformity exists in this respect. The largest mass hitherto discovered was, as has been mentioned already, found within a few inches of the surface. It is evident that the thin stratum which contains the metal will be buried at different depths, by variable quantities of alluvial earth, that are accumulated over it by causes still in operation; and consequently, that the depth at which the stratum happens to be met with in any given place, is no criterion of its richness. Nor does the fact that this fortunate discovery was made at a lower level than ordinary, afford any encouragement

encouragement to work lower than the usual depth. It might interest geological curiosity, however, to learn the nature of the strata below the gold deposit, although I do not know that the existence of this furnishes any reasonable grounds for supposing that there are other similar deposits below it. I could not find that any search had been made with such an expectation, except in a single instance. Near the spot where the largest mass was found, the earth was penetrated a few feet below the gold bed. Immediately beneath this was a thin layer of green sand, and next a similar layer of a bright yellow sand. These had a very handsome appearance, but neither of them seemed to contain any thing more precious than mica.

The terms on which the proprietors of the mines permit them to be worked, vary with the productiveness of the earth which is worked. Some of the miners rent for a fourth of the gold found; some for a third, and others claim half, which is the highest premium hitherto paid. The avarage product at Reed's mine was not more than sixty cents a day to each labourer; but the undertakers are buoyed up with the hope of some splendid discovery, like those which have occasionally been made.

The mines have given some peculiarities to the state of society in the neighbouring country. The precious metal is a most favourite acquisition, and constitutes the common currency. Almost every man carries about with him a goose quill or two of it, and a small pair of scales in a box like a spectacle-case. The value, as in patriarchal times, is ascertained by weight, which, from the dexterity acquired by practice, is a less troublesome mode of counting money than one would imagine. I saw a pint of whisky paid for by weighing off three grains and a half of gold.

The greatest part of the gold collected at these mines is bought up by the country merchants at 90 or 91 cents a pennyweight. They carry it to the market-towns, as Fayetteville, Cheraw, Charleston, and New-York. Much of this is bought up by jewellers; some remains in the banks; and a considerable quantity has been received at the mint of the United States. Hence it is not easy to ascertain the precise amount which the mines have afforded. The value of that portion received at the mint before the year 1820, was 43,689 dollars. It is alloyed with a small portion of silver and copper, but is still purer than standard gold, being 23 carats fine. (Bruce, Mineral. Jour. i. 125.

It will probably appear evident to geologists, from the foregoing statements, that the gold of N. Carolina occurs in a diluvial formation. Such indeed seems to be its usual bed; and, in

in this respect, it resembles the gold countries of South America, of England, of Scotland, of Ireland, and of Africa. (Buckland, Rel. Diluv. 218—20.)

I have already adverted to an impression entertained by the inhabitants of our gold country, that the precious metal exists somewhere in the vicinity in an ample bed or vein, from which the pieces found are derived. It may not be uninteresting to inquire whether we can obtain any light respecting its origin.

1. *Is it brought down from the sources of the rivers?*

That this is not the case is evident, because it is not found merely in the beds of the rivers, but also in the neighbouring grounds, and that too whether the ground be plain or hilly. The formation in fact, crosses over hill and dale, and frequently the earth which is obtained on the hill side, or on the summits of an elevation of one or two hundred feet above the beds of the streams, is rich in metal. It is found on both sides of the Yadkin, and in the bed and throughout all the branches of Rocky River. It is evident, then, that the rivers do not bring down the gold from their sources, but that they cut through a stratum containing it, which covers like a mantle, an extensive tract of the country through which they flow, and that they bring the precious metal to view by separating it from its stony matrix.

2. *Did the present lumps and grains ever form parts of large masses in a continued bed or vein?*

It has been already remarked that the present aspect of these pieces is such as would naturally result from collision among the siliceous fragments that accompany them. Impressions of sand and gravel, or even imbedded sand, might, it is true, be the result of fusion in a bed of sand; but the appearance is not that which arises from fusion under such circumstances, the cavities being superficial, forming impressions or indentations, while there is no appearance in any specimen that I have seen of a grain of sand enveloped by the mass*. But if the present appearance of these lumps and grains be owing to attrition, and the formation be, as we have supposed, a deposit from water, then we must regard them as the remains of larger pieces, reduced in size by collision with the accompanying minerals, but not as parts of very large masses which have been torn up and broken into fragments. The same cause that would be sufficient to break up into fragments the accompanying gravel, would not break up large masses of gold into smaller pieces, since gold is soft and malleable, and not brittle

* Vide Kirwan's Geological Essays, 402.

and

and unyielding like quartz. The effect of running water and dashing rocks would be to wear down the pieces of gold and compress them, but not to break them. The fine flakes and dust of gold may be conceived to have been produced in this manner; and the relative quantity of dust may afford some means of judging of the original size of the lumps and grains from which it was derived. In the gold of this formation, but little dust, comparatively, is saved, although more, I believe, might be saved by a more improved process of working. At present the greater part collected is in the state of grains, or small scattered lumps. The inference is, that this gold existed originally, that is, before its removal to its present position, in pieces somewhat larger than those found at present, but still of a moderate size. Whether these pieces lay contiguous to one another in a large vein, or whether they were flattened abroad in individual masses, it is, perhaps, impossible to decide. The fact that small veins have been found traversing quartz, favours the idea that this was the original mode of existence.

There are some circumstances which induce the belief, that the materials of the deposit itself were derived from the great slate formation before mentioned. The green mud may be supposed to have been formed out of the chlorite and argillaceous rock, with which the formation abounds; the greenstone pebbles correspond with a class of rocks of the same formation; and the quartzose fragments answer well in appearance to the larger fragments, that are profusely scattered over the ridges of the slate country. Moreover, two masses of gold, each weighing several pennyweights, have been found in the county of Orange, over the same formation, 60 or 70 miles north of the gold region. Hence might be derived some faint hopes of finding the gold in native veins or beds; but still these may have been in the "fountains of the great deep" that were broken up.

If we suppose that gold dust is universally derived from diluvial action on lumps of the same metal, it will account for two well known facts;—first, the very general diffusion of particles of gold among the sands of all countries; and, secondly, the circumstance of many rivers that were anciently auriferous, having now ceased to be so; as the Tagus, Po, and Pactolus (Kirwan, Geological Essays, 402.) This author also adds, that it appears by the testimony of Disdown that some of the rivers of France were much more abundantly auriferous in former ages than they are at present. The dust derived from diluvial action may be conceived to be exhausted or washed out in the course of ages, while there is now no process going forward for supplying the waste.

LXI. *Notices*

LXI. *Notices respecting New Books.*

The Elements of Medical Chemistry; embracing only those Branches of Chemical Science which are calculated to illustrate or explain the different Objects of Medicine; and to furnish a Chemical Grammar to the Author's Pharmacologia. *Illustrated by numerous Engravings on Wood.* By John Ayrton Paris, M.D. F.R. & L.S., Fellow of the Royal College of Physicians of London, &c. London, 1825. 8vo. pp. 586: with an appendix of tables.

THIS work, Dr. Paris informs us,—in a spirited "dialogue between the author and a practitioner who is about to direct the medical studies of his son" which serves as a preface to it,—is founded upon the notes from which he formerly lectured. "And as my pupils were entirely medical," he observes, "it was my care to collect all the chemical facts of professional interest, to conduct the student to a knowledge of their principles by the shortest path, and to remove from his road every adventitious object that might obstruct his progress, or unprofitably divert his attention."

In these objects, we think, Dr. Paris has been eminently successful in the work before us: it is an excellent and admirably arranged selection of facts from the best authorities on the subjects of which it treats, combined and applied in such a manner as evinces the author to be a master of the science of "Medical Chemistry." Nor is it deficient in original views, where alone in such a work they were to be expected,—on the relations of chemistry to physiology and pathology: and we cannot do better, perhaps, than quote, as a specimen of the author's style and manner of treating his subjects, part of his statement "In what manner Chemistry is subservient to Medicine," &c. After stating the specious objection to the utility of chemical science in physiology, founded upon the axiom "that animated bodies are not only enabled to resist all the laws of inanimate matter, but even to act on all around them in a manner entirely contrary to those laws," he enters into the following examination of this argument and some of its bearings:—

"8. Nor must it be forgotten that in some of the functions of the living body, the vital energy would seem rather to correspond in its action with chemical affinity, than to oppose or supersede its influence; and several of the senses may be said to owe their energies to the perfection of organs which are entirely constructed upon philosophical principles. Thus re the laws of optics and acoustics in active operation during the

the exercise of the visual and auditory apparatus; and it is a question whether some chemical action is not established by the agency of sapid bodies upon the epidermis of the mucous membrane of the mouth: it is, at least, seen evidently in some cases, as in the effects of vinegar, the mineral acids, a great number of salts, &c. By the same agents similar effects are produced upon dead bodies; and Dr. Majendie thinks that to this species of combination the different kinds of impression made by sapid bodies may be fairly attributed, as well as the variable duration of such impressions. Nor is it reasonable to deny that many of our remedies may act by a chemical action on the alimentary canal: alkalies are thus frequently serviceable, by clearing the primæ viæ of superfluous animal matter, which they effect by forming with it a soluble compound. If the origin of animal heat cannot be satisfactorily traced to a strictly chemical source, its maintenance, distribution, and regulation may at least be shown to depend upon the agency of those laws which alike govern the temperature of inert matter. Do we not perceive that every animal suffering from diminished temperature, instinctively diminishes the surface of its body, which is in contact with the cooling medium? Man under such circumstances is seen to bend the different parts of his limbs upon each other, and to apply them forcibly to the trunk *. It will be also seen, when we come to consider the nature of capillary action, that many of the phænomena of living bodies, which have been erroneously attributed to the action of the living principle, may be satisfactorily explained by the simple operation of this attractive force. The absurdities of the chemical and mechanical sects have undoubtedly driven the modern physiologist into a mischievous scepticism with regard to the influence of physical causes upon a living animal. John Hunter, even to associate whose name with error will be regarded by many as an act little short of impiety, has repeatedly attributed to the specific effect of life, actions that ought to be solely referred to the powers belonging to inanimate matter. In the same manner an objection to impute to the physical property of elasticity certain phænomena exhibited by membranous structure, has led Bordieu†, Bichat ‡, Blumenbach §, and others, to assign to it a peculiar vital power,

* Children and weak persons often take this position when in bed. It would therefore be improper to confine young children in swathing clothes so as to prevent the necessary flexion.

† Recherches sur le Tissu Muqueux, § 70.

‡ Traité des Membranes, pp. 62, 101, 133.

§ Instit. Physiolog. § 40, 59.

whose

whose existence has neither been proved by experiment, nor rendered probable by analogy.

"9. But there is another point of view in which the same question may be advantageously regarded. The pathologist will have to contemplate the living powers in various states of languor and decay, when they will be found incapable of wholly resisting the laws which govern inanimate matter; and we shall learn, during the progress of the present work, that in certain conditions of the human body, several of the fluids will undergo the same chemical decompositions as would take place in the laboratory. The same observation will apply to the agency of mechanical causes. In a state of perfect health, the fluids of the body will not descend to the inferior parts, agreeably to the law of gravitation, because the vital power opposes itself to this hydraulic phænomenon, and with an energy in direct proportion, as it would seem, to the robust and vigorous state of the individual; for if the person be reduced by disease, this tendency will be only imperfectly resisted; the feet in consequence will swell. The following experiment of Richerand may be here related, to show how greatly the power manifested in the living body of resisting, with more or less success, the influence of physical force, is enfeebled by disease. He applied bags filled with very hot sand all along the leg and foot of a man who had just undergone the operation for popliteal aneurism; the artery was tied in two places under the ham. Not only was the usual coldness which follows an interruption of the circulation thus prevented; but the extremity, so managed, acquired a degree of heat much greater than the ordinary temperature of the body. The same apparatus, when applied to a healthy limb, was unable to produce that excess of caloric, obviously in consequence of the energy of life opposing such an effect.

"10. Mr. Earle has published an interesting paper*, to prove that, when a limb is deprived of its due share of vitality, it is incapable of supporting any fixed temperature, and is peculiarly liable to partake of the heat of surrounding media. The cases which are adduced prove also that a member so circumstanced cannot without material injury sustain a degree of heat which would be perfectly harmless, or even agreeable, to a healthy part: thus, the arm of a person became paralytic, in consequence of an injury of the axillary plexus of nerves, from a fracture of the collar-bone. Upon keeping the limb for

* Cases and Observations, illustrating the Influence of the Nervous System in regulating Animal Heat: by H. Earle, Esq. Published in the 7th volume of the Transactions of the Medico-Chirurgical Society.

 nearly

nearly half an hour in a tub of warm grains, which were previously ascertained by the other hand not to be too hot, the whole hand became blistered in a most alarming manner, and sloughs formed at the extremities of the fingers. In a second case, the ulnar nerve had been divided by the surgeon for the cure of a painful affection of the arm; the consequence of which operation was, that the patient was incapable of washing in water at a temperature that was quite harmless to every duly vitalized part, without suffering from vesication and sloughs.

"11. It follows then that the pathologist may frequently derive important conclusions from the doctrines of chemistry, although we must deeply regret that this department of medical knowledge, like that of physiology, should have suffered from too hasty generalization: but let no one attempt to tolerate his apathy, or to encourage his despair, by a reference to failures which have arisen on the one hand from a deficiency of knowledge, and on the other from errors which are to be solely attributed to a perversion of it;—let him rather seek encouragement in the contemplation of those useful improvements which have been derived from the judicious application of chemical science, and which are daily augmenting the resources of the intelligent physician, and tending to diminish the aggregate of human suffering."

Dr. Paris concludes his work with announcing his intention of following it by a volume on Animal Chemistry, having been prevented from proceeding beyond Vegetable Chemistry, it would seem, by the bulk to which the work had already extended;—and we hope he will speedily fulfil this design.

Recently published.

The Third Part of Volume XIV. of the Transactions of the Linnæan Society of London, has just appeared; and the following are its contents:

Observations on the Natural Affinities that connect the Orders and Families of Birds. By Nicholas Aylward Vigors, Esq. A.M.—Descriptions of two Species of Antelope from India. By Major-General Thomas Hardwicke.—Description of a new Species of Tailed Bat (*Taphosous* of Geoff.) found in Calcutta. By Major-General Hardwicke.—Anatomical Observations on the Natural Group of *Tunicata*, with the Description of three Species collected in Fox Channel during the late Northern Expedition. By William Sharp MacLeay, Esq., A.M.—A Description of a new Species of *Scolopax* lately discovered in the British Islands: with Observations on the *Anas glocitans* of Pallas, and a Description of the Female of that Species. By N. A. Vigors, Esq. A.M. —A De-

—A Description of such Genera and Species of Insects, alluded to in the "Introduction to Entomology" of Messrs. Kirby and Spence, as appear not to have been before sufficiently noticed or described. By the Rev. William Kirby, A.M. —Description of *Cowania*, a new genus of Plants; and of a new Species of *Sieversia*. By Mr. David Don, Libr. L.S.— Description of the *Buceros galeatus* from Malacca. By Major-General Thomas Hardwicke.

ANALYSIS OF PERIODICAL WORKS ON NATURAL HISTORY.

Zoological Journal.

No. V. of this useful work has just appeared, commencing the second volume; together with a Fasciculus of Supplementary Plates to vol. i. The Number contains the following articles:—The Rev. Mr. Kirby's Introductory Address, explanatory of the views of the Zoological Club, delivered at its foundation, reprinted from the Philosophical Magazine.—Some further Remarks on the nomenclature of *Orthoptera*, with a detailed description of the genus *Scaphura;* also by Mr. Kirby. This paper is illustrated by figures of the generic characters of *Scaphura*, and by a coloured figure of *S. Vigorsii.*—Observations on the Structure of the Throat in the genus *Anolis;* by Mr. Bell with figures: On the utility of preserving facts relative to the habits of animals; by Mr. Broderip.—Additional observations upon a Fossil found in Coal Shale, and the description of a Palate found in Coal, near Leeds; by Messrs. J. D. C. Sowerby and E. J. George: with a figure.—Notice of the Occurrence of some rare British Birds; by Mr. Yarrell.—Descriptions of some new and rare *Volutes*, by Mr. Broderip. In the introduction to these descriptions Mr. Broderip ably vindicates from some current aspersions the class of scientific collectors of subjects of Natural History, of which he forms so distinguished a member. The only Volute described by him which is absolutely new is *V. rutila*, a beautiful species, of which a coloured plate is given: it is characterized as "V. testâ ovato-oblongâ, rufescente, maculis subtrigonis, confluentibus, crocco-rubris variâ; spirâ brevi, suturâ simplici; apice papillari, subgranulato: anfractu basali tuberculis elongatis armato, fasciisque 2, latis, interruptis, rutilis, ornato; columellâ 4-plicatâ. Habitat in Ocean. Austral.?"—Continuation of Mr. Vigors's Sketches in Ornithology, of which the subjects are, a group of *Psittacidæ* known to the ancients, which Mr. Vigors has erected into a genus called *Palæornis*; and a new Brazilian genus of *Falconidæ*, to which he applies the name of *Gampsonyx.*—Mr. Kirby on a pair of remarkable horned mandibles of an Insect; with a figure.—Mr. French on Instinct, Essay III., On the Specific Constitution of the Brute Mind, and its Modifications under Human Influence.—Characters and Descriptions of several Brazilian *Thamnophili;* by Mr. Swainson.—Descriptions of some shells, belonging principally to the genus *Chiton*, observed on the coast of Argyleshire in 1824; by R. T. Lowe, Esq.; with a coloured plate.—List of the British *Vespertilionidæ;* by Mr. Gray: from this it appears that there are at least eleven species of Bats in this country.—Descriptions of some hitherto uncharacterized Brazilian Birds; by Dr. Such: among these is a new genus of *Laniadæ* denominated *Gubernetes:* the species described, *G. Cunninghami*, is figured.—Analyses of Books, among which is Spix's *Simiarum et Vespertilionum Brasiliensium Species Novæ.*—Zoological proceedings of Societies.—The Sup-

Supplementary Plates are 8 in number, and are beautifully executed: Tabb. I. to III. illustrate Mr. Vigors's account in No. IV. of the genus of *Psittacidæ*, which he has called *Platycercus;* Tab. IV. represents *Psittacus pyrrhopterus*, also described by that naturalist; and Tab. V. to VIII. are *Thamnophili*, decribed by Dr. Such.

LXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 5.—PROFESSOR BARLOW, F.R.S., in a letter to Mr. Herschel, communicated a paper On the magnetism imparted to iron bodies by rotation.

May 12.—A paper was read On the magnetism produced in an iron plate by rotation; by S. H. Christie, Esq. A.M. F.R.S.

May 19.—A paper was read, entitled A Description of the Transit-Instrument by Dollond, erected at the Observatory at Cambridge; by Robert Woodhouse, A.M. F.R.S.: and Professor Buckland communicated a paper On the fossil elk of Ireland; by Thomas Weaver, M.R.I.A., &c.

LINNÆAN SOCIETY.

May 3.—The President (Sir J. E. Smith) in the Chair.—Professor Fr. A. Bonelli, and M. Ch. Sigismund Kunth were chosen to fill the two vacancies in the list of Foreign Members. The remaining part of Messrs. Sheppard and Whitear's paper on the Birds of Norfolk and Suffolk was read: it concluded with a table of the times of the migration of various species, as observed at several places in those counties for a series of years. A further portion of Dr. Hamilton's Commentary was also read.

May 24.—The Anniversary of the Linnæan Society was held on this day at one o'clock, at the Society's house in Soho Square, Sir J. E. Smith, President, in the Chair; when the following were chosen Officers for the ensuing year.

Sir James Edward Smith, M.D. F.R.S., &c. *President;*
Edward Forster, Esq. F.R.S. &c. *Treasurer;*
James E. Bicheno, Esq. *Secretary;*
Richard Taylor, Esq. *Assistant Secretary.*

The Vice-presidents of the preceding year were re-appointed: viz. Samuel, Lord Bishop of Carlisle, LL.D V.P.R.S. F.A.S.; A. B. Lambert, Esq. F.R.S. A.S. and H.S.; Wm. Geo. Maton, M.D. F.R.S. and A.S. Edward, Lord Stanley, M.P. F.H.S.;

The following gentlemen were appointed to fill up the vacancies in the Council:—James E. Bicheno, Esq.; Edward Horne,

Horne, Esq.; Charles König Esq.; Rev. Thomas Rackett; James F. Stephens, Esq.

The anniversary dinner of the Society at the Freemasons' Tavern was well attended. The presence of Sir J. E. Smith in improved health added much to the enjoyment of the day. Among those who addressed the meeting on subjects interesting to the lovers of Natural History, were the venerable bishop of Carlisle; Lord Stanley; T. A. Knight, Esq., President of the Horticultural Society; the Rev. Professor Buckland, President of the Geological Society; and the Rev. Dr. Fleming. Numerous expressions of respect and cordial esteem towards A. MacLeay, Esq., who has for many years most ably filled the office of Secretary, were called forth on the occasion of his quitting England for a time, to fill the important office of Colonial Secretary in New South Wales.

ASTRONOMICAL SOCIETY.

May 13.—The reading of Mr. Henry Atkinson's elaborate communication on the subject of Refraction was concluded. In the course of this paper the author has collected and arranged a great variety of meteorological observations, made in different seasons, and at different parts of the world, for the purpose of enabling him to ascertain the mean temperature at the equator and in different latitudes, as well as the law of variation in the temperature of the air at different heights above the level of the sea. From these data he has deduced formulæ, by the use of which the *computed* and *observed* mean temperatures in any given place, or at any given height, appear to agree in a remarkable manner. His next inquiry is, to ascertain the law by which the *height* and the *elasticity* of the air is connected; and also the relation between the *elasticity* and *density* at any given height. These inquiries are guided by observations and experiments that have been made and published by men of eminence in this department of science. The reasoning and deductions are founded on acknowledged facts; and hypothesis furnishes no part of the data from which the *tables*, founded on these investigations, are computed. *Astronomical* observations supply no portion of the materials which form the basis of the computations, but all the results are obtained by formulæ depending on *optical principles;* so that the near agreement of the quantities contained in these tables (when properly collected) with those given by the most approved modern tables of refraction proves that the various formulæ by which these quantities were obtained are founded in nature, as well as happily applied. The atmosphere is divided into a variety of strata, and each stratum has its appropriate formula for determining its share of mean refraction;

fraction; and when the different portions belonging to the different strata are put together in succession, they constitute such an arrangement of quantities as proceed by a regular gradation, or very nearly so; and nothing but a close examination of the differences can detect that the whole succession has not depended on one continued formula. Besides the atmospheric refractions adapted as corrections for celestial observations, the author has applied one of his formulæ successfully to determine the terrestrial refraction as it has reference to two objects standing in different elevations: so that whether this memoir be considered as a meteorological, geodetical, or astronomical communication, it cannot but be regarded as copious, elaborate, and interesting.

There was also laid before the meeting an account of observations made at Paramatta, in New South Wales, by Major-Gen. Sir Thomas Brisbane, K.C.B. Governor, &c.; communicated in a letter to Francis Baily, Esq. President of this Society. These refer to the solar eclipse on January 1, 1824; to several occultations of fixed stars by the Moon; to stars observed with the Moon near her parallel; to observations before and after the superior conjunction of Venus with the Sun, July and August 1824; to observations on the planet *Uranus* near the opposition in July 1824; and to observations on two comets, one of which was not observed in Europe.

Next there was read a report On the properties and powers of an altitude and azimuth circle constructed by Edward Troughton, and divided by T. Jones; drawn up by the Rev. W. Pearson, LL.D. F.R.S. and Treasurer to this Society. The peculiarities of the construction of this fine instrument cannot be adequately described in an abstract. But some estimate may be observed of its accuracy from stating, that by comparing the *mean* latitude of South Kibworth rectory (Leicestershire) with each and all of *sixteen* separate determinations, it does not differ more than one second and one-tenth from the extreme latitude; that the true obliquity of the ecliptic at the December solstice 1824, as determined by this instrument, was 23° 27′ 44″,01; while the mean of the determinations of Delambre, Brinkley, and Bessel is 23° 27′ 44″,55. Observations on the pole-star, and another determination of the obliquity of the ecliptic, by a method suggested by Dr. Brinkley, serve still further to confirm the character of the instrument for accuracy, and the value of such an instrument when used by a skilful, scientific, and experienced observer.

The reading was commenced of a paper On the construction and use of some new tables for determining the apparent place of about 3000 principal fixed stars; drawn up, at the request of the Council, by the President.

ROYAL ACADEMY OF SCIENCES OF PARIS.

March 7.—The director-general of bridges and causeways requested the Academy to hasten the work required of it by government, on high-pressure steam-engines.—M. Ferrand presented a new drawing of his marine lever.—M. de Lacépède made a verbal report on M. Virey's History of the Human Race.—M. Mathieu, in the name of a commission, read a report on the *Panorograph* of M. Puissant.—M. Arago exhibited to the Academy an apparatus, showing in a new form the action which magnetized and non-magnetized bodies mutually exert on each other. In his first experiments, M. Arago proved that a plate of copper, or of any other solid or liquid substance, placed under a magnetic needle, exerts on it an action which immediately affects the amplitude of its oscillations, without sensibly altering their duration. The phænomenon to which he now directed the attention of the Academy may be regarded as the converse of the preceding. As a needle in motion is stopped by a plate of copper, &c. at rest, M. Arago conceived that a needle at rest would be moved by the plate when in motion. If a plate of copper, in fact, be made to revolve with any determinate velocity beneath a magnetized needle contained in a vessel perfectly closed, the needle will no longer take its usual position: it stops without the magnetic meridian, and so much the further from that plane as the rotation of the plate is more rapid. If the rotary motion be sufficiently rapid, the needle turns continually round the wire on which it is suspended, whatever be its distance from the revolving disc.

LXIII. *Intelligence and Miscellaneous Articles.*

COLLECTION OF SHELLS.

IN consequence of Mr. Swainson's going abroad, he has requested us to make known his desire of parting with his entire collection of shells.—It is not our intention to speak of the scientific interest that it is known to possess, having been formed by its possessor for the express purpose of writing a general history of testaceous animals. It contains but few of those well known, but costly shells, which are usually seen in other cabinets, neither is it rich in British Testacea; but the number of species from all other parts of the world is immense; and in this respect the collection may vie with any now is this country. Their number, or even that of the specimens, has never been ascertained; but some idea of both may be formed, when it is stated that the collection occupies about eighty good-sized drawers.

FALLING STAR SEEN AT MID-DAY.

"On the 13th of August 1823, at a quarter past eleven in the forenoon, as I was employed in measuring the zenith distances of the pole-star to determine the latitude, a luminous body passed over the field of the universal instrument telescope, the light of which was somewhat greater than that of the pole-star. Its apparent motion was from below upwards; but as the telescope shows images in an inverted position, its real motion, like that of every falling body, was from above downwards.

It passed over the telescope in the space of a second, or a second and a half, and its motion was neither perfectly equal nor rectilinear, but resembled very much the unequal and somewhat serpentine motion of an ascending rocket, from the unequal burning of the charge, and the irregular re-action of the stream of air issuing from it on the atmospheric air. It was thus evident that this meteor moved in our atmosphere, but it must have been at a considerable height, since its angular motion was so slow. This is perhaps the only instance in which a shooting-star has been seen at mid-day in clear sunshine."—*Hansteen.* [*Edin. Phil. Journ.*]

LENGTH OF THE PENDULUM UNDER THE EQUATOR.

The expedition to the equator (for the purpose of determining this important problem) was set on foot by Mr. Goldingham, under the encouragement of Sir Thomas Munro and Sir Stamford Raffles, in 1821. In 1822 the party under Capt. Crisp arrived at Bencoolen; and after some time occupied in searching for an eligible spot, stationed themselves on a small island named Gaunsah Lout, in January 1823. The latitude of the island was 0° 1′ 48″78. The observations and the experiments were continued till the end of March, and were very numerous and laborious. The details form the bulk of the report, a folio of 268 pages, including, however, a series of observations to determine the geographical position of a number of places in the vicinity;—the result, affecting the main object of the expedition, giving the length of the pendulum at Gaunsah Lout, was inches 39·02125994.—*Asiatic Journ.*

INTERESTING VOYAGE OF DISCOVERY.

On Thursday his majesty's ship Blossom, Capt. F. W. Beechey, sailed from Portsmouth upon her interesting voyage of discovery and survey in the Pacific, previously touching at Rio Janeiro, to land dispatches for his excellency Sir Charles Stuart. After visiting Pitcairn Island, Otaheite, Easter and Friendly

Friendly Islands, and settling indisputably the position of all the islands with which that neighbourhood abounds, we understand the Blossom is to proceed to Bhering's Strait, and, if the season admit of it, to proceed round Icy Cape (which has not been effected since Capt. Cook's discovery of it) along the northern shore of America towards Hecla and Fury Strait, for the purpose of falling in with Capt. Franklin or Capt. Parry; and if Capt. Beechey find the sea open, it is most likely he will not omit so fortunate an opportunity of accomplishing this desirable object. We understand also, that the Blossom is to complete the survey of the coast of America in such parts about Bhering's Strait as are imperfectly known; and after having rendered Capt. Franklin the assistance he may require, she is to proceed entirely upon discovery, directing her route for such purpose towards those parts of the Pacific which are the least known or frequented. She is furnished with a large supply of presents, for the purpose of bartering with the islanders, and has on board a handsome present for the king of Otaheite and the king of the Sandwich Islands. The Lords of the Admiralty have appointed Mr. Tradescant Lay to be naturalist to the expedition; and we look for the most interesting results from the several purposes intended to be accomplished by it. The surveying and opening a communication with the Friendly Islands may eventually prove of considerable importance. Upwards of 12,000 acres of land have been some time inclosed and put into cultivation of cotton; the samples of which are pronounced inferior only to the Sea Island cotton, and much superior to Egyptian.—*Morn. Chron.* May 24.

EARTHQUAKE IN ICELAND.

Accounts from Iceland to the middle of March say, that they have had a long and severe winter, which began in September. At the beginning of January there were dreadful hurricanes, which were followed by inundations in several parts of the island. Several shocks of an earthquake were also felt in Norder Gossel; and on the 20th of January, a very smart one in Suderland.

This is about the same time that Santa Maura, in the Ionian Islands, was destroyed by an earthquake.

Many travellers and shepherds perished in the heavy snow which fell during the violent storms in Norderland.—*Copenhagen*, April 21.

MEXICO.

Accounts have been received of the safe arrival of Dr. Coulter at the Real del Monte mines. We may expect from his zeal and ability important additions to our knowledge of the

Natural History of Mexico. The affairs of the mines wear a very favourable appearance.

TARTARIZED ANTIMONY.

We extract the following instructive analysis of tartarized antimony, by Mr. R. Phillips, from the *Annals of Philosophy*.

A. 100 grains of brilliant small crystals of tartarized antimony reduced to powder were heated during eight hours at the temperature of 212°; they lost only 2·1 grains; but as bitartrate of potash retains the water of crystallization when exposed to a much greater heat, I subjected 100 grains of tartarized antimony reduced to powder to a higher temperature. Taking the mean of several experiments, I found that the salt lost 7·38 per cent. by several hours' exposure to a sand heat. When one portion, which had lost 7·4 per cent. in this way, was heated by a spirit-lamp, so as to suffer a further diminution of 0·4, it was decomposed, and becoming of a brown colour, it emitted the smell of decomposing tartaric acid. I consider, therefore, 7·4 per cent. as the quantity of water.

B. I attempted to ascertain the quantity of oxide of antimony in two different modes. First, I decomposed a solution of 100 grains of the crystals by carbonate of soda, assisted with heat; the mean of two experiments gave 41·35 per cent.; but the alkalies being imperfect precipitants of antimony, I treated the solution afterwards with sulphuretted hydrogen, which gave a mean of 2·8 of precipitate dried at a moderate temperature, and which I conceived to be hydrosulphuret of antimony composed of 1 atom of sulphuretted hydrogen 17 + 1 atom protoxide of antimony 52 = 69; if then 69 contain 52 of oxide 2·8 = 2·11, which, added to 41·35, gives 43·46 as the quantity contained in 100 parts of the salt.

C. After this, and in order to confirm the above statement, I treated two solutions, each of 100 grains, of tartarized antimony with sulphuretted hydrogen gas; the precipitates after being dried on a sand heat gave a mean of 51·25 grains. Now, if we admit, as above supposed, that this precipitate is hydrosulphuret of antimony, and of which it possesses the appearance, tartarized antimony contains only 38·6 instead of 43·46 of protoxide of antimony as by Experiment B, for 69 : 52 :: 51·25 : 38·6. It may be further observed that the quantity of precipitate obtained in C, as well as the inference as to the quantity of oxide which it contains, agree very nearly with the previous determinations of Thenard.

These discordant results, repeatedly obtained, puzzled me exceedingly, and I adopted two modes of determining the quantity of oxide existing in the dried hydrosulphuret.

D. I dis-

D. I dissolved 45·6 grains of protoxide of antimony in a solution of bitartrate of potash, and then precipitated it by sulphuretted hydrogen: after washing and drying in the same mode as before, the precipitate weighed 52·8 grains; and, as it had the appearance of being an hydrosulphuret of antimony, I suspected that it was subhydrosulphuret consisting of 1 atom of sulphuretted hydrogen, and 2 atoms of oxide of antimony, on which supposition I ought to have obtained 53·05 of precipitate instead of 52·8; and it will be observed that supposing this to be the true constitution of this substance, the results of B will agree with those of C as nearly as 43·46 to 44·04, for subhydrosulphuret of antimony must consist of $52 \times 2 + 17 = 121$, and 121 : 104 :: 51·25 : 44·04.

E. Further to examine this view of the subject, I dissolved 100 grains of sulphuret of antimony and 100 grains of the precipitate in question in separate and equal quantities of muriatic acid, and decomposed the solutions with similar portions of water; the precipitate from the sulphuret weighed 86·7 grains, and that from the hydrosulphuret 87 grains; and as in 1 atom of subhydrosulphuret, 17 of sulphuretted hydrogen, and 16 of oxygen = 33, would supply the place of 2 atoms of sulphur = 32 in 2 atoms of sulphuret of antimony, it is evident that equal weights of these compounds should yield nearly equal quantities of precipitate by solution in muriatic acid and the affusion of water.

F. The nature of this precipitate was then examined by heating 50 grains in a small flask by a spirit-lamp, and I found to my surprise, that it was readily converted into black sulphuret of antimony, losing only 1·2 grain of water. It appears, therefore, that instead of a subhydrosulphuret as I had suspected, the precipitate was sulphuret, containing a small quantity of hydrosulphuret, but yet sufficient to give so much colour as to conceal the nature of the sulphuret. The difficulty of the case was increased by the fact already alluded to, viz. that 2 atoms of oxygen and 1 atom of sulphuretted hydrogen are so nearly equal in weight.

G. As then 50 of the precipitate contain 48·8 of sulphuret of antimony, 51·25 the whole quantity obtained, C must contain 50·02 = 43·35 of protoxide of antimony, which is the quantity contained in 100 grains of crystallized emetic tartar.

We have thus obtained 7·4 as the quantity of water, and 43·35 as the weight of the protoxide of antimony; and having found, as already mentioned, that crystals of tartarized antimony are obtained even from the last portions of the solution in preparing the salt, the remainder of 49·25 will give the weight

weight of the bitartrate of potash in 100 parts, or it consists of

Bitartrate of potash (anhydrous)	49·25
Protoxide of antimony	43·35
Water	7·40
	100·00

Calculating its constitution, according to the weight of the atoms already mentioned, tartarized antimony will appear to be a compound of

		In 100 parts.
1 atom of bitartrate of potash	180	49·58
3 atoms of protoxide of antimony (52 × 3)	156	42·97
3 atoms of water (9 × 3)	27	7·45
	363	100·00

POWDER FOR MAKING BEER.

A German chemist has invented a dry mass or powder for making ale, extracted from a vegetable, which is very cheap and to be had everywhere, and by which can be produced a wholesome and agreeable ale or small beer of any degree of strength, of the best and purest quality, without adding anything but the proper quantity of water, at every season, in greater or less proportions, and at a very cheap rate.

To the Editor of the Philosophical Magazine and Journal.

Dear Sir,

I beg to acknowledge my obligation to Mr. Stockton, for his readiness in furnishing you with the annual depth of rain at New Malton since the year 1816, and by that means correcting my statement in page 237, in which I had only given the mean annual depth for the last *three* years, as he supposes, not having had by me your Magazine for a longer period. I am glad to find by Mr. Stockton's complimentary remark, that the diminution of the mean depth for a longer series of years does not lessen the value of my article. I am, yours, &c.

Gosport, May 25th. 1825. WILLIAM BURNEY.

LIST OF NEW PATENTS.

To Augustin Louis Hunout, of Brewer-street, Golden-square, for certain improvements, communicated from abroad, in artillery, musketry, and other fire-arms.—Dated 23d April 1825.—6 months to enrol specification.

To Thomas Alexander Roberts, of Monford-place, Kennington Green, Surry, for a method of preserving potatoes and other vegetables.—23d April.—6 months.

To Samuel Ryder, of No. 40, Gower-place, Euston-square, coach-maker, for an improvement in carriages by affixing the pole to the carriage by a new invented apparatus.—28th April.—2 months.

To Daniel Dunn, of King's-row, Pentonville, Middlesex, manufacturer of essence of coffee and spices, for his apparatus for separating the infusion of tea or coffee from its grounds or dregs.—30th April.—6 months.

To

To William Davis, engineer, of Leeds, and of Gloucestershire, for his improvements in machinery for reducing wool into slivers or threads of any desired length, unlike worsted, namely, presenting more numerous hair points projecting from the surface of the slivers or threads.—7th May.—6 months.

To Thomas Hill the younger, of Ashton-under-Line, Lancashire, land-surveyor and engineer, for improvements in the construction of railways, and tram roads, and in carriages to be used thereon and on other roads.—10th May.—6 months.

To Edward Elliss, of Crexton, near Rochester, lime-merchant, for his improved brick, or substitute for brick, manufactured from a material hitherto unused for or in the making of bricks.—14th May.—6 months.

To Samuel Pratt, of New Bond-street, Middlesex, camp-equipage manufacturer, for a manner of combining wood and metal, so as to form rails or rods adapted to the manufacture of bedsteads, cornices, and other works where strength and lightness are desirable, which he denominates Union or compound rods.—14th May.—6 months.

To John C.C. Raddatz, of Salisbury-square, Fleet-street, in consequence of a communication made to him by Ernst Alban, of Rostock, M.D., for improvements in steam-engines.—14th May.—6 months.

To Jean François Gravier, of Cannon-street, London, for a method, communicated from abroad, of regulating the emission or flow of gas from portable reservoirs, and increasing the security of such reservoirs.—14th May.—6 months.

To Thomas Dyke, of Broadway, near Ilminster, Somersetshire, dissenting minister, for an apparatus to prevent the overturning or falling of carriages—14th May.—2 months.

To Alexander Galloway, of West-street, London, engineer, for a machine for the forming and moulding of bricks and other bodies usually made from clay, plastic, or any of the usual materials.—14th May.—6 months.

To William Grimble, of Cowcross-street, Middlesex, for improvements in apparatus for distilling spirituous liquors.—14th May.—6 months.

To Edward Garsed, of Leeds, flax-spinner, for improvements in machinery for hacking, combing, or dressing flax, hemp, and other fibrous materials.—14th May.—6 months.

To Henry Oswald Weatherley, of Queen Ann-street, Mary-le-bone, Middlesex, for his apparatus or machinery for splitting, cutting, or cleaving of wood, and forming and securing the same in bundles.—14th May.—6 months.

To Goldsworthy Gurney, of Argyle-street, Hanover-square, Middlesex, surgeon, for his apparatus for propelling carriages on common roads, or on railways.—14th May.—6 months.

To John Young, of Wolverhampton, cooper, for his improvements in the construction of locks for doors and other purposes.—14th May.—6 months.

To James Fox, of Plymouth, rectifying distiller, for an improved safe to be used in the distillation of ardent spirits.—14th May.—2 months.

To Charles Macintosh, of Crossbasket, in the county of Lanark, esq., for a new process for making steel.—14th May.—6 months.

To John Badams, of Ashted, near Birmingham, chemist, for a new method of extracting certain metals from their ores and purifying certain metals.—16th May.—6 months.

To Isaac Reviere, of 315, Oxford-street, Middlesex, gun-maker, for an improved construction, arrangement, and simplification of the machinery by which guns, pistols, and other fire-arms are discharged.—20th May.—6 months.

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport, Mr.* CARY *in London, and Mr.* VEALL *at Boston.*

Days of Month, 1825.	Gosport, at half-past Eight o'Clock, A.M. Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Clouds. Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Height of Barometer, in Inches, &c. Lond. 1 P.M.	Bost. 8¾ A.M.	Thermometer. London. 8 A.M.	Noon.	11 P.M.	Boston. 8¾ A.M.	Rain. London.	Boston.	Weather. London.	Boston.	Wind.
Apr. ☽ 26	29·60	55	49·05	63	SE.	...	0·210	1	1	1	...	1	1	1	29·56	29·50	50	60	51	46·5	...	...	Fair	Cloudy	E.
27	29·27	56	......	73	SE.	0·20	·160	...	...	1	...	...	1	1	29·25	29·15	50	59	48	47	...	·02	Stormy.	Rain	E.
28	29·30	57	......	65	S.	...	·130	1	1	1	...	1	1	1	29·40	29·12	50	60	51	51	...	·77	Fair	Fine	SE.
29	29·43	56	......	64	SE.	...	......	1	1	1	...	1	...	...	29·58	29·22	51	63	52	53	...	·19	Fair	Rain	S.
30	29·68	57	......	61	S.	·25	·135	1	...	1	...	1	1	1	29·80	29·43	52	60	49	51·5	...	·17	Fair	Cloudy	E.
May 1	29·67	53	49·00	65	S.	...	·420	1	1	1	...	1	1	1	29·74	29·50	49	57	47	51·5	...	...	Fair	Cloudy, rain *p.m.*	E.
○ 2	29·64	56	......	67	S.	...	·245	1	...	1	...	1	1	1	29·66	29·30	50	60	52	54	1·25	·30	Fair	Fine, lightning with	SW.
3	29·68	57	......	65	SW.	·20	......	1	1	1	...	1	1	1	29·86	29·35	53	60	50	55	...	·23	Fair	Cloudy [rain *p.m.*	W.
4	29·90	58	......	61	S.	...	·410	1	1	1	...	...	1	1	29·96	29·60	51	66	60	59	...	...	Fair	Cloudy	SE.
5	29·78	57	......	84	E.	...	·365	1	1	1	...	1	1	1	29·85	29·55	60	66	60	57·5	...	·59	Fair	Rain	Calm
6	29·78	63	......	67	S.	·25	·200	1	1	1	...	1	1	1	29·84	29·50	63	70	60	62	...	...	Fair	Cloudy, thunder and	Calm
7	29·81	58	49·25	68	W.	...	·005	1	1	1	...	1	1	1	29·90	29·40	57	67	56	61	...	...	Fair	Cloudy [light^s^. *p.m.*	Calm
8	29·92	60	......	67	SW.	...	·005	1	...	1	...	1	1	1	29·96	29·53	59	66	55	59	...	...	Fair	Fine, rain at night	SE.
9	30·12	58	......	68	NW.	·45	......	1	1	1	...	1	1	1	30·10	29·70	55	66	54	51	·20	·10	Fair	Cloudy	NW.
10	30·06	56	......	61	N.	...	·145	...	1	1	...	1	1	...	30·07	29·78	55	66	53	50·5	...	...	Fair	Cloudy	Calm
11	29·98	59	......	60	E.	...	·255	1	...	1	...	1	1	1	30·02	29·27	53	66	50	52	...	...	Fair	Cloudy	W.
12	29·78	58	......	66	SE.	·40	·085	...	...	1	...	...	...	1	29·86	29·65	52	58	49	51	...	...	Rain	Cloudy, rain at night	E.
13	29·85	56	49·25	72	NW.	...	......	1	1	1	...	...	1	1	29·96	29·75	50	49	42	50	...	·27	Rain	Fine	E.
14	30·13	52	......	61	NE.	...	......	...	...	...	...	1	1	...	30·20	29·93	47	55	41	51	...	...	Fair	Fine	N.
15	30·19	50	......	54	NE.	·50	......	1	...	1	...	1	1	...	30·15	30·00	46	57	44	51	...	...	Fair	Fine	N.
16	30·08	52	......	60	N.	...	·010	...	...	1	...	...	1	...	30·10	29·33	45	54	46	47·5	1·20	...	Fair	Cloudy, rain *p.m.*	NE.
17	30·12	56	......	55	NE.	...	......	...	1	1	...	...	1	1	30·20	29·90	47	56	47	49·5	...	·16	Cloudy	Rain	E.
18	30·28	56	......	51	NE.	·50	......	...	1	1	...	1	1	...	30·31	30·00	46	57	47	47·5	...	...	Fair	Fine	NE.
19	30·28	53	49·60	56	NE.	...	......	...	1	1	...	1	1	...	30·32	30·00	47	56	46	53	...	...	Fair	Fine	E.
20	30·26	56	......	53	E.	...	......	1	1	1	...	...	...	...	30·24	30·00	46	58	50	56	...	...	Fair	Fine	E.
21	30·17	60	......	53	E.	·80	......	1	...	1	...	1	...	...	30·20	29·95	46	64	50	52	...	...	Fair	Fine	E.
22	30·10	62	......	54	SE.	...	......	1	1	1	...	1	...	...	30·10	29·80	51	68	55	57	...	...	Fair	Fine	E.
23	30·02	58	......	67	E.	...	......	1	1	1	...	1	1	...	29·95	29·63	57	74	60	63·5	0·00	...	Fair	Fine, rain with thun^r^	SW.
24	29·84	65	......	60	SW.	...	......	1	...	1	...	1	1	...	29·80	29·35	60	68	55	60·5	...	·20	Fair	Fine [& light^s^. *p.m.*	W.
☽ 25	29·69	62	49·90	60	SW.	·90	......	1	1	1	...	1	1	1	29·66	29·23	56	66	56	62·5	...	...	Fair	Fine	S.
Aver.:	29·880	57·07	49·34	62·7		4·45	2·780	22	19	29	...	23	25	18	29·92	29·58	52	62	51	53·7	2·65	3·00			

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

30th JUNE 1825.

LXIV. *On the largest Mass of Meteoric Iron which has yet been discovered in Europe. By Dr.* J. Nœggerath, *and Dr.* G. Bischof, *of Bonn.**

DR. NŒGGERATH, on his former travels in the Eifel, had been informed from various quarters, that in the neighbourhood of Bitburg, and close to the mill of Albach, there lay a mass of solid and perfectly malleable iron of several thousand pounds weight; but that it was not known how it came there. In the course of his travels, however, he never passed this spot. During the visit of Dr. Chladni at Bonn, in the year 1817, this subject was again brought under consideration; and as it was thought deserving of further inquiry, application was made to the Royal Provincial Counsellor M. Simonis, for information respecting it. That gentleman returned the following answer, dated from Bitburg, the 19th December 1817:

"About ten years ago a miller at Albach, when widening a road, discovered a square mass of iron weighing 3300 pounds, which was afterwards sold to a smelter for 16 crowns and a half. Before its removal, an engineer in the French service came from Luxenburg to examine it. I accompanied him to the spot, and we struck off several pieces with a hammer, which the engineer took with him. The iron was by no means brittle, but could only be separated from the mass in thin flakes. On examining the vicinity, I found that the arable fields, to the extent of several acres, were covered with iron slag, which convinced me that in former times a smelting furnace must have been worked near this spot; and that the mass of iron above mentioned must have had its origin from that circumstance. As no running water is to be found on the hill, the furnace must have been worked by wind, or by horses, or men."

Chladni, in consequence of these notices, was led to class this mass among the problematical meteoric irons. (*Vide* his Treatise on Fire-balls, &c. Vienna, 1819, p. 353.) But as

* From Schweigger's *Journal*, N. R. Band xiii. p. 1.

M. Simonis's account did not favour the idea, no further attention was paid to the subject, until Chladni published the following remarks in Gilbert's Annals, 1821, part viii. p. 342.

"The mass of solid iron discovered near Bitburg (and of which I was informed by Professor Nœggerath,) is undoubtedly of meteoric origin; for it appears from the American Mineralogical Journal, vol. i. No. 4, p. 219, that Colonel Gibbs (the same officer who took some pieces with him,) found them to contain nickel. According to his statement the mass was malleable, and weighed 2500 pounds. It was round, in consequence of the peasants having struck pieces off. Some parts were semi-hard, others gave fire with steel. The whole mass agreed with those found in Louisiana with regard to compactness and colour, its less oxidability than common iron, its proportion of nickel, its toughness, and its isolated appearance.

"The American Journal could not be obtained here for further reference."

After the appearance of this notice, every exertion was made to learn what had been done with the above mass; and it was at length discovered that it had been smelted at a furnace in the district of Schöndorf; and subsequently, in 1824, Dr. Nœggerath, in company with M. Von Westphalen, proceeded to the place where the iron had been found; and received an account from the persons residing near the spot, agreeing in substance with the above.

They then visited M. Müller, the owner of the iron-works where the mass had been smelted, and obtained the following information. "About ten years since," that gentleman stated, "I heard that a large piece of iron had been found near Bitburg, and that the proprietor of the soil was willing to dispose of it. I therefore sent for samples, which appeared to me to be iron in almost a pure state; and I accordingly bought the whole at three francs per cwt. (being 33 or 34 cwt.), and was at a great expense to bring it to the forge. It was not of any decided form, but quite compact, without any apparent admixture of extraneous substances, and seemed as if it had been melted together; it was almost malleable, and fragments could be struck from it but with difficulty. It had the appearance of smelted ore, which on successive casting presents a knotted and wavy surface; in short, it seemed to have been formed by the melted metal being poured out. The whole piece was smelted with immense labour, for the purpose of forming smaller ones; but when the latter were brought under the hammer, they flew into small pieces, and could not possibly be brought to weld." It was therefore of no service; and M. Müller, to prevent its substitution for pure metal, ordered it to be

buried

buried among the rubbish. The workmen having pointed out the spot, several pieces were dug up, and identified beyond the possibility of doubt. These pieces were forwarded by Dr. Nœggerath to Bonn, and Dr. Bischof undertook to analyse them. Specimens were also sent to the Royal Mining Department at Berlin, to M. Karsten, of the same place; to Professor Hausmann, in Göttingen; to Professor Von Leonhard, at Heidelberg; to Dr. Chladni and M. Bergemann, in Berlin. Though unable to present an analysis of the original mass, it will be interesting to the scientific reader to obtain a correct description and analysis of this substance after being smelted as above mentioned.

The pieces were from six to eighteen inches in diameter, with a thickness of from one-third to two inches of the real metallic mass. The lower side exhibited evident traces of having been cast on the floor of the forge, fine sand and fragments of stone and coals adhering to it: the upper surface in most of the pieces was covered with a coat of slag about one inch thick. The colour of the fracture is light steel-gray, approaching to that of white tin, or similar to the appearance which white crude iron frequently presents. When polished, the colour is nearer that of steel than of iron. The fracture has a metallic lustre; but on account of the peculiar texture it is not equal, and in some parts is merely glimmering. The fracture is also uneven, of a fine grain, which is looser and less compact than in steel.

In the mass which we are describing there were no veins.

In regard to the air-bubbles which appear in the mass, some difference is observable in the different pieces. Some may be called spongy; and throughout the whole of the metal, larger or smaller, irregular, but generally elongated bubbles are disseminated.

In other pieces, which are on the whole more compact, these bubbles run through the whole mass, almost in the form of tubes; but they appear invariably in rectangles on the larger sides of the pieces; so that when they are polished on these sides they are covered with roundish pores, which are sections of these tubular bubbles. Pieces of both kinds filed smooth and polished, and then repeatedly treated with nitric acid, showed no traces of latent regularity of texture, or of the so-called Widmannstädt figures, as is observed in all solid masses of meteoric iron when they have not been subjected to a subsequent change like that under consideration.

In regard to hardness, this mass was exactly equal to the gray granular crude iron produced from the brown iron ore which is used in the royal foundry at Sayn, near Neuwied,

for musket-barrels; the one cannot be scratched by the other. However, white crude steel-iron from the royal foundry at Hamm, near Altenkirchen, was found to scratch the metallic meteoric mass.

This iron is not very tough. Pretty large flakes may be struck into several pieces by a few strokes of a moderately sized hammer; yet the mass is in some degree ductile, and may be easily filed. At a red and at a white heat it was found to be very brittle, as was the case when worked in its original state at the foundry of Pluwig. A moderate stroke of the hammer on a glowing piece immediately scatters it into innumerable sparkling small fragments, part of them in the form of dust.

The specific gravity of a piece of this kind, which was most compact and rather sparingly interspersed with the above-mentioned tubular bubbles, was 6·859 at 61° Fahrenheit.

The metallic mass is apparently attracted as powerfully by the magnet as common iron; but of polarity there were no signs whatever.

On boring the masses a smell of sulphuretted hydrogen was perceived; this was particularly the case when the masses were first dug up and their pores filled with moisture. In this latter state, even the fresh-fractured surfaces showed a strong tendency to oxidation, since in a few hours afterwards they were covered with separate spots of a green colour, and at last presented one entire coat of rust.

The slag or scoria is of a grayish-black colour, the fracture glistening, of a small and uneven grain, filled more or less with roundish bubbles, the largest being three-fourths of an inch in length, covered here and there with small fine iron-black crystals with metallic lustre, which are mostly indistinct, with rounded surfaces, sides and angles; but triangular faces may still frequently be discerned, which in their combination seem to point to an octahedral form, and on the whole exhibit a great similarity to magnetic iron ore. The mass of the slag scratches glass, and is attracted by the magnet.

It would answer no useful purpose to subject the meteoric substance to a quantitative analysis, since it must have undergone a change in the proportions of its constituent parts in the process through which it passed in the foundry; especially as a great quantity of slag was separated from it by that operation: but should any fragments of the original substance be found, (as we have reason to hope,) we might then have it in our power to publish an analysis stating the quantity of its component parts. Experiments were therefore instituted to discover those substances which have been found in meteoric iron.

I. *Analysis*

I. *Analysis of the Iron.*

1. A fragment of the iron, being well cleansed from the scoria, was treated with nitro-muriatic acid. The decomposition took place very rapidly; and in a short time the greatest part of the iron was dissolved, whilst a delicate gray powder was separated from it.

2. A portion of the filtered light-brown solution was saturated with caustic ammonia, and the fluid filtered off: it had then a clear bright-blue colour, without the least shade of violet *.

3. A portion of this blue solution being decomposed by muriatic acid, gave a dirty yellow precipitate, with the addition of ferro-prussiate of potash, which proves the existence of nickel in it. Another portion was saturated with sulphuric acid, and a stream of sulphuretted hydrogen gas passed through it. After some time a precipitate was formed of a brown colour, and of the appearance of dust; but in such small quantities that it could not be taken from the filter; and it was therefore impossible to examine it more closely, or to discover what metal it contained. I shall, however, make an attempt to procure it in greater quantity.

4. The greatest part of this blue solution was evaporated to dryness. There remained an apple-green salt, which being heated in a platinum crucible until the sal ammoniac was volatilized, left a bright-brown powder, which underwent no change *per se* before the blowpipe. With borax added to a considerable quantity of it, it first presented the dark-brown bubble, or pearl, described by Berzelius, which on cooling assumed a reddish colour: but on a continued exposure to the flame, although the changes took place which Berzelius describes, yet no signs or traces were visible of a colour indicating cobalt. The absence of the latter was further evinced by moistening paper with a muriatic solution of this powder, and then warming it.

5. The gray powder which had not been dissolved by the nitro-muriatic acid was washed and dried; and a part of it being heated in a platinum crucible, sulphur was disengaged and volatilized. The residuum on cooling was quite black, and dissolved entirely in hot muriatic acid. This solution contained nothing but iron; for when decomposed by am-

* This test, employed by Klaproth, although found by subsequent experiments to be unsatisfactory for the purpose of separating nickel from iron, was nevertheless made use of here; because it was only intended to present a qualitative analysis, or, in other words, to discover the constituent parts, without reference to their proportional quantities.

monia

monia a colourless fluid remained, which on drying left nothing but clear sal ammoniac. The remainder of the powder which was not ignited, was dissolved in nitro-muriatic acid with the aid of heat, when flakes of sulphur were separated from it. A precipitate of graphite could not be discovered,—a proof of the absence of carbon. That a portion of the sulphur was oxidized by the nitro-muriatic acid, was shown by testing the first solution with muriate of barytes.

6. Although the quantitative definition of the proportion of sulphur is of no particular consequence, for the reasons above specified, yet it appeared of some interest to us not to overlook this particular altogether. For this purpose, 100 grains of the iron were submitted to the action of boiling nitro-muriatic acid; but a perfect solution was not attainable, an earthy powder being separated, weighing 1·80 grains, and in which were contained 0·47 grains of sulphur.

It is beyond a doubt that this earthy part proceeded from some slag which had not been driven off, but still remained in the iron, in the pores before alluded to; for the former piece, to which the acid was gradually added, left no earthy sediment. This was, however, of a close texture; whereas the piece on which the last experiment was tried was very spongy, and plainly showed the scoria, with which many of the pores were filled up. On this account the earthy residuum was not analysed.

From the solution thus obtained the sulphuric acid was precipitated by muriate of barytes. The sulphate of barytes, washed and dried, weighed 18·60 grains, in which the proportion of sulphur is 2·57 grains. Add to this the above 0·47 grains, and the total is 3·04 per cent.

7. It now remained to ascertain the existence of chrome, which John discovered in several meteoric irons *. For this purpose, the above precipitate of oxide of iron obtained by ammonia was washed, till no more re-action on muriate of ammonia was observable in the water, then mixed in a porcelain cup with a sufficient proportion of nitre, and heated till the latter was dissolved. The aqueous solution of the mass neutralized by means of nitric acid had not a yellow colour, and no orange-yellow precipitate was occasioned by adding proto-nitrate of mercury. Another quantity of the oxide of iron, heated in a platinum crucible with carbonate of soda, gave as little indication of the presence of chromic acid. This mass of meteoric iron, therefore, contains no chrome. As the aqueous solution had not a green tinge, but was quite co-

* See his *Chemische Schriften*, book vi. p. 292.

lourless,

lourless, this seemed to indicate that the precipitate of iron contained no manganese. The following experiment, however, was made, for greater certainty.

8. A third quantity of the well-washed oxide of iron was dissolved in muriatic acid, and after proper neutralization with ammonia was precipitated by means of succinate of soda; but the fluid decanted from the precipitate showed no manganese. It is to be observed, that a part of the fluid evaporated to dryness left an entirely white mass of salt, which was found to be common salt with the excess of succinate of soda. In this residuum there was no trace of nickel; which is the more remarkable, as several of our most distinguished analysts have found that the oxide of nickel precipitated at the same time with oxide of iron by the ammonia, cannot be entirely extracted from the precipitating medium, although the latter be added in ever such abundance. In conformity to this, we might have expected to find oxide of nickel in the oxide of iron employed for the above experiment; but it was not the case.

II. *Analysis of the Scoria.*

The scoria, reduced to a fine powder in the steel mortar, was dissolved in hot nitro-muriatic acid, leaving an earthy powder, which it could answer no purpose to examine. This solution showed scarcely a trace of nickel; so that during the smelting of the iron mass, the iron only, and not the nickel, had been converted into scoria. A part of the sulphur, however, entered the scoria; for muriate of barytes produced a precipitate in the solution. The oxide of iron precipitated by ammonia, being well washed, was examined in the above manner for chrome but not a trace of it was to be detected; which shows that chrome had not passed over to the scoria during the smelting, as might have been expected.

The result, therefore, of these experiments is, that the iron mass consisted of iron, nickel, and 3·04 per cent of sulphur; and that the scoria contained iron with an almost imperceptible trace of nickel and sulphur: the earthy parts are to be considered as accidental. These facts give rise to the following remarks:

1. All the accounts respecting the appearance, former shape, and other mineralogical peculiarities of this mass, agree in favouring the opinion that it is of meteoric origin.

2. The proportion of nickel found in it by chemical analysis is the more corroborative of this opinion, as nickel is found in all meteoric iron, but has not been met with in any earthy mineral substances: besides this, the district of Bitburg does not afford any minerals which contain nickel, nor has it been found

found in any of the Prussian and Belgic provinces on the left bank of the Rhine.

3. As no sulphur has yet been found in the solid meteoric masses of iron in the analyses hitherto made, the proportion of 3·04 per cent in a mass which had been smelted is the more striking, as it must be supposed that during the latter process some parts were volatilized and others transferred to the scoria. We must leave it undecided, whether this proportion of sulphur was combined equally with the whole mass of metal, or appeared only as a constituent part of sulphuret of iron, such as is found in meteoric stones, properly so called. The latter supposition seems the most probable, and agrees with Gibbs's account of the different degrees of hardness in this mass, but not with M. Müller's assertions with respect to its homogeneity.

It must however be observed, that sufficient attention was not paid to individual sprinklings in the mass, which might have been found in some parts when detached and not in others. The native iron in the form of branches, the spaces being filled with olivine, found by Pallas in Siberia, and which forms in some degree the transition between real meteoric stones and solid native iron, contains in some parts sulphuret of iron. Laugier, on analysing it, found 5·2 per cent of sulphur; and subsequently John has proved by an analysis, in conjunction with Laugier, that the malleable part of this mass was free from sulphur; but that the brittle parts, partly consisting of olivine, contained sulphur, probably in the form of finely disseminated sulphuret of iron. Now as the appearance of sulphuret of iron, such as is usual in meteoric stones, is proved in that of Pallas, which is the medium between them and solid native iron, it need not surprise us if it be once found in the latter.

In this supposition the iron mass in question, containing 3·04 per cent of sulphur, would have afforded 8·16 per cent of magnetic pyrites.

4. The great brittleness of the smelted mass could only proceed from the sulphur; since all natural as well as artificial combinations of iron with nickel present a tough and ductile alloy. This also agrees with the remark of Hassenfratz,—that iron with copper-nickel is with difficulty smelted, and cannot be soldered at all; it is remarkably brittle at a red heat, and in some degree so when cold.

As copper-nickel contains mostly sulphur, independent of arsenic, which does not render the iron brittle, the above is easily accounted for.

5. The absence of carbon in the mass of native iron discovered

vered by Smithson Tennant at the Cape of Good Hope, is a further proof that the Bitburg mass was not originally produced from the furnace; the more so, since it was merely smelted, and not actually refined.

6. In justification of the title of this memoir, we beg leave to observe that the solid iron-mass found by Pallas weighed 1400 pounds; that which fell in 1751, near Hradschina, only 71 Vienna pounds; that at Elbogen, in Bohemia, 191 pounds; that of 1814, at Lenarto, in Hungary, 194 pounds.

It is true that the mass which is the subject of the present observations, although between 3300 and 3400 pounds, is greatly surpassed by those found in various parts of America, which are stated at 14,000, 30,000, and 40,000 pounds.

GIBBS *on the Native Meteoric Iron found near Bitburg. Communicated by Dr.* NŒGGERATH.

It could not but be interesting to me to obtain a particular account from Colonel Gibbs respecting this substance, as he seems to have been the only scientific person who had seen it in its original state. In the foregoing article we were not able to institute a comparison between his account and our own; but since then, I have received, through the kindness of Professor Hausmann of Göttingen, a copy of Col. Gibbs's description inserted in the American Mineralogical Journal, conducted by Archibald Bruce, vol. i. no. 4, p. 218. After some remarks on the meteoric iron found in Louisiana, Col. Gibbs proceeds thus:

"The appearance of this interesting specimen reminded me of a mass which I met with in the year 1805, in a mineralogical excursion through the Ardennes in France. It lay on the way to Bitburg, in the department *des Forets*, and weighed, as was supposed, 2500 pounds.

"The country-people told me that it had formerly stood on the top of a neighbouring hill, and had been rolled down to where it then was. The difficulty of breaking it for the furnace was the means of preserving it, and it is probably there still. As I had fortunately taken a specimen, I analysed it, and found nickel likewise; which proved it to be native iron. The mass had a globular form, the peasants having cut off the edges. It was in some places semi-hard; other parts gave sparks with steel: it was perfectly compact, and in other respects similar to the iron-mass found in Louisiana.—I must observe," continues Col. Gibbs, "that the mass found in Siberia differs in some particulars from those of Louisiana and Bitburg.

burg. The two latter resemble the former in colour, and in containing nickel; they are in like manner tenacious, and found in isolated masses. That of Louisiana contains a trace of carbon; and to judge from the hardness of a part of the Bitburg mass, it must have contained some carbon also, like those of South America, and Magdeburg. Those of Mexico and Peru are found in volcanic districts like that of Bitburg, which lies in the region of the extinguished volcanoes of the Rhine *. The most striking difference is, that the masses in France (alluding to Bitburg, which then belonged to France) and Louisiana appear compact, without any thing like a glassy substance or a cellular texture; yet they may have cavities within, like those of South America, and are perhaps only compact on the surface through attrition."

On the very small Octahedrons in the Scoria of the Smelted Meteoric Iron of Bitburg. By Dr. J. Nœggerath.

Very small octahedral crystals were found in the above scoria, and were considered by Professor Bischof and myself as magnetic ironstone. Professor Hausmann, however, noticed that these crystals were exactly similar to those which he described in his treatise on crystallized iron scoria. (*Vide* Moll's Journal of Mining and Smelting, vol. iii. page 39.) M. Hausmann allows that he was at first deceived in the nature of these crystals. "As magnetic iron constitutes a principal part of the scoria, and the crystals of the latter are also octahedral," observes M. Hausmann, "I considered them as crystallized magnetic iron ore, as they had a black colour, and I did not examine them very minutely."

The same causes led me into error, especially as the crystals in the scoria of the meteoric iron are but small, and scarcely to be distinguished but by their prominence; and on the dark ground of the scoria they showed no appearance of being pervious to the light.

The crystals are really of a vitreous nature, translucent, of a dark-green colour, inclining to yellow, of a conchoidal fracture. They scratch glass, and are not regular, but rectangular octahedrons. I did not make any chemical experiments with them, and cannot therefore decide whether they agree with those examined by M. Hausmann. I refer to that gentleman's treatise; and only observe that the analysis presented oxide of iron, silica, lime, and alumina.

* This is not exactly correct. Volcanic appearances are not found within a considerable distance of Bitburg, which lies among secondary rocks. New red sandstone, gypsum and muschelkalk, are the principal strata.

Laugier's

LAUGIER'S *latest Analyses of Meteorites from Poland. Communicated by Dr.* NŒGGERATH.

The preceding account had already been sent to press, when I received the last number (6[e] Ann, 2[e] Cah.) of *Memoires du Muséum d'Histoire Naturelle*, in which Laugier inserts the analysis of a meteoric mass of iron similar to that of Pallas. It also contained sulphur. Laugier examined two varieties of this meteoric iron found in 1809 at Brahin, in the district of Rziezyca-Minsk. He says, that it perfectly resembles that of Siberia, being in like manner full of cavities, which seem to be covered with a vitreous greenish-yellow substance, which was easily detached, and was considered to be olivine.

The variety of this iron, which he calls the blueish, was found on analysis to contain

Pure iron	87·35
Silica	6·30
Nickel	2·50
Magnesia	2·10
Sulphur	1·85
Chrome	0·50
	100·60

The white variety contained

Pure iron	91·50
Silica, somewhat tinged by iron	3·00
Nickel	1·50
Magnesia	2·00
Sulphur	1·00
	99·00

Laugier also analysed the following meteoric stones from Poland:—

1. One which fell on the 30th of June 1820 at Lixna, near Dunabourg. Its outward appearance was like that of most aërolites, and it contained, as many of them do, small shining globules, which could not be pulverized, and on being separated by a magnetic bar, composed a full fourth of the whole mass. 100 parts of this aërolite with its shining globules contained,—

Oxide of iron	40·00
Silica	34·00
Magnesia	17·00
Sulphur	6·80
Alumina	1·00
Nickel	1·50
Chrome	1·00
Lime	0·50
Traces of copper and manganese	
	101·80

2. A

2. A stone that fell on the 30th of March 1818, at Zaborzyca, and which, not containing globules like the foregoing, could be easily pulverized.

Laugier found in 100 parts,

Oxide of iron	45·00
Silica	41·00
Magnesia	14·90
Sulphur	4·00
Lime	2·00
Nickel	1·00
Alumina	0·75
Chrome	0·75
Trace of manganese	
	109·40

LXV. *On the Discovery of an almost perfect Skeleton of the* Plesiosaurus. *By the Rev.* W. D. Conybeare, *F.R.S. M.G.S.**

I AM highly gratified in being able to lay before the Society an account of an almost perfect skeleton of *Plesiosaurus*†, a new fossil genus, which, from the consideration of several fragments found only in a disjointed state, I felt myself authorised to propound in the year 1821, and which I described in the Geological Transactions for that and the following year. It is through the kind liberality of its possessor, the duke of Buckingham, that this specimen has been placed for a time at the disposal of my friend Professor Buckland, for the purpose of scientific investigation.

At the period of my former communications it was natural and even just that in the minds of many persons interested in such researches, much hesitation should be felt in admitting the conclusions of an observer who was avowedly inexperienced in comparative anatomy; and there might have then appeared reasonable ground for the suspicion that, like the painter in Horace, I had been led to constitute a fictitious animal from the juxtaposition of incongruous members, referable in truth to different species. But the magnificent specimen recently discovered at Lyme has confirmed the justice of my former conclusions in every essential point connected with the organization of the skeleton.

The only material error which I have to correct relates to the bones which I supposed to be the radius and ulna: but with

* From the Geological Transactions, New Series, vol. i.

† Some philological objections having been made to the composition of the word *Plesiosaurus*, I beg to state that it is formed on the very same principle as the words Ισαγγελος, Ισοδενδρος, &c., all of approved classical use.

with regard to the other parts of the skeleton, in assigning to the same animal the heads and vertebræ, which had at that time never been found in connexion, and whose actual relation was therefore regarded by many as equivocal, in indicating the order and place of the several kinds of vertebræ, and in tracing the osteology of the humero-sternal parts, my opinions have received full confirmation. In the attempted restoration of the paddle also (though professedly given only on conjecture) a very considerable approximation to the true structure of the part will be found, considering the very imperfect materials afforded by the fragments which had then been obtained.

But in addition to these particulars, which in all their material features were correctly stated, the specimen now exhibited presents others of a most novel and interesting character, not to have been anticipated previously to the discovery of a skeleton, the whole exterior portion of whose vertebral column was perfect. I particularly allude to the neck, which is fully equal in length to the body and tail united; and which surpassing in the number of its vertebræ that of the longest-necked birds, even the swan, deviates from the laws which were heretofore regarded as universal in quadrupedal animals, and the cetacea. I mention this circumstance thus early, as forming the most prominent and interesting feature of the recent discovery, and that which in effect renders this animal one of the most curious and important additions which geology has yet made to comparative anatomy.

I now proceed to the details in the usual order.

Head.—The present specimen, and another of this part only, in possession of Miss Philpot, confirm the restoration attempted from the distorted head figured in Plate XIX. of the first volume of the second series of the Geological Transactions; and the latter extends our knowledge by exhibiting distinctly the occipital portion. We now also learn for the first time, that the head of this animal was remarkably small, forming less than the thirteenth part of the total length of the skeleton; while in the *Ichthyosaurus* its proportion is one-fourth. This proportional smallness of the head, and therefore of the teeth, must have rendered it a very unequal combatant against the latter animal; but the structure of its neck may perhaps be considered as a compensating provision, supplying it with the means of security and of catching its prey.

Vertebræ.—The distinctions between the cervical and caudal vertebræ have been fully and correctly stated in my former communications; but I had not at that time observed more than twelve of the cervical, whereas the present specimen exhibits about thirty-five, or, including the anterior dorsal,

sal, which were placed before the humerus, and bore only five ribs, forty-one*. This great increase of the number of joints in the neck is the more remarkable from the rigour with which nature appears, in most cases, to have enforced the law of a very limited number. In all quadrupedal animals, in all the mammalia (excepting only the tridactyle sloths, which have nine) the series is exactly seven; and so strict is the adherence to this rule, that even the short and stiff neck of the whale, and the long and flexible neck of the cameleopard, are formed out of the same elementary number; the vertebræ in the former instance being extremely thin and anchylosed together, and in the latter greatly elongated. Reptiles possess only from three to eight cervical vertebræ; birds, approaching in this more nearly to the present species, but still falling greatly short of it, have from nine to twenty-three†, the number being the greatest in the swan. The *Ichthyosaurus* appears to have possessed about eighteen cervical vertebræ. In fishes the ribs commence almost immediately behind the head.

The views of Geoffroy de St. Hilaire,—that nature in the organization of the animal frame has caused the sternal portion to shift its position along the vertebral column,—seem to derive an important corroboration from the structure of this animal: but it is remarkable, that whereas the sternum holds a mean position in quadrupeds, and is thrown forward in fishes and backwards in birds, yet its position in this instance assimilates the *Plesiosaurus* less to fishes, though destined to move in the same element‡, than to birds, and exhibits at the sáme time a very wide departure from the type of the Saurian tribe. Al-

* It is difficult to assign the exact demarcation between these subdivisions of the column; because the inferior lateral or hatchet-shaped processes of the cervical vertebræ (which in this animal greatly resemble those of the crocodile) gradually become elongated, and assume almost insensibly the character of false ribs.

† The sparrow is said to possess only nine cervical vertebræ (Cuvier's *Anatomie Comp.*). In aquatic birds the length of the neck, as well as the number of the cervical vertebræ, generally exceeds what we observe in the land birds, this construction enabling the former to procure sustenance in their own peculiar element.

‡ The *Testudo longicollis*, an inhabitant of fresh-water and a native of Australasia (see Shaw's Zoology, vol. iii. p. 62), is the most remarkable among the tortoises for length of neck; and the figure of this animal in the work referred to will serve to illustrate what in the *Plesiosaurus* must have been the external appearance of this part when covered with integuments. It would be very desirable to ascertain, from an examination of the skeleton, whether this species has more than the usual number of cervical vertebræ. Most of the tortoise tribe have the power of extending their necks considerably, especially the *Testudo ferox* (see Shaw. vol. iii. p. 65), whose neck, when exserted, is equal in length to the shell. By darting out this it is enabled to make even birds its prey.

though

though the number of the cervical vertebræ is thus unexampled, yet the length of the neck is nearly rivalled by another of the reptile class, namely, the land tortoise. The length is in this case concealed by the anterior extension of the shell; the neck, however, notwithstanding its elongation, has only eight vertebræ. The general proportions of the tortoise, its length of neck, shortness of tail, and the smallness of its head, are in some degree analogous to what we observe in the *Plesiosaurus;* but the structure of the head and teeth of the latter, and its want of shell, entirely negative the idea of its being intimately allied to the tortoise, and decidedly connect it with the Saurian order.

It will be necessary to subjoin a few words on the inferior hatchet-shaped processes which may be seen depending on either side from the lower part of the cervical vertebræ. Most animals present traces of these processes; they are particularly prominent in many of the long-necked quadrupeds, and in birds project into a long styloid branch; a rudiment of these may be observed in man, but I am not aware that any particular name has been assigned to them*. They have been sometimes confounded with the transverse processes, to which they often form a wing-like appendage. These processes are important, as serving to determine the number of the cervical vertebræ, and as affording very close analogies between the *Plesiosaurus* and the crocodile; in both these animals these inferior hatchet-shaped processes are exactly similar in figure, and form separate pieces attached to the body of the vertebræ by a double stem. In the figures given of the cervical vertebræ in my former memoir, this stem alone, and the double suture which receives it, could, from the imperfect state of the specimens, be represented; but I then expressed my conviction that the structure resembled that of the same part in the crocodile, and my conjecture is now verified.

The thirty-five anterior vertebræ of the *Plesiosaurus* exhibit these processes distinctly characterized, and are therefore beyond all doubt cervical; in the six following the processes become lengthened, and gradually lose their hatchet-shaped extremity, assuming rather the form of false ribs, and should therefore perhaps be classed as anterior dorsal; but the whole forty-one are clearly placed before the pectoral extremities. In the crocodile there are seven cervical vertebræ with hatchet-

* Dr. Macartney, in his Anatomy of Birds, says, "The transverse processes of the vertebræ of the middle of the neck spread forwards, and send down a styloid process of some length."—"The anterior styloid processes are but little observable in the rapacious and passerine tribes, the parrot, &c.; but they are very marked in the long-necked birds."

shaped processes, and three anterior dorsal with false ribs before the humero-sternal portion. Since flexibility must evidently be the end of this great multiplication of the joints, it may perhaps excite surprise that the joints, instead of articulating as in birds by cylindrical surfaces, should have their contiguous faces nearly flat, which must have allowed a less freedom of motion between each vertebra; but it may be answered, that the increased number of the joints compensated for the stiffness of each.

Dorsal Vertebræ.—I have nothing to add to my former remarks on this part of the column: the greater part of these, in the splendid specimen from Lyme, are removed from their place, and are scattered over the mass of shale in which they are imbedded. In consequence of this accident, we are admitted to a full view of the ribs and sterno-costal arcs and pelvis, which remain undisturbed. Fourteen large ribs may be counted; and twenty-one dorsal or lumbar vertebræ appear dispersed, though their exact original number cannot be ascertained. The last of these lies over the pubis, and has, close to it, a short false rib.

Twenty-three caudal vertebræ are remaining; and as about three of the extreme ones appear to be wanting, we may probably assume this part at about twenty-six joints: the whole vertebral column then will number about 90 joints; viz. 35 cervical, 6 anterior dorsal, 21 dorsal and lumbar, 2 sacral, and about 26 caudal. The proportions of the whole of these parts will stand nearly thus: taking the head as 1, the neck will be as 5, the body as 4, and the tail as 3, the total length being, as was before remarked, thirteen times that of the head.

The chevron bones beneath the tail are finely exhibited; but this part, having been fully described in my former papers, suggests no new remarks, excepting that its shortness must have prevented its being used, as in fishes, as an instrument of impulsion in a forward direction, and that it was therefore probably employed only as a rudder to steer the animal by horizontal flexure, or by a sudden vertical stroke to elevate or depress it while swimming through the water.

The anterior sternal portion is greatly concealed by the vertebræ and ribs lying over it; these might be carefully removed and replaced, and the structure of this important part ascertained. From several imperfect specimens which I have examined, it appears to have been complicated in its structure, and nearly to have resembled that of the *Tupinambis.*

The posterior part of the sternum consists of a central bony arc, crescent-shaped, and swelling in the middle; to its horns are applied two sterno-costal branches, which appear as usual

to

to have been connected with the extremities of the ribs by cartilages: the nice adaptation of these parts is beautifully displayed in the specimen.

The pelvis is finely displayed, and resembles the usual type of this part in reptiles, of which the turtle perhaps affords the best example for comparison with the fossil: the ilium is reduced to a long and slender bone, which might, if seen detached, be mistaken for the os pubis; that of several species of turtle is exactly similar. The ischium is like that of most reptiles; and the pubis, as is also common in this class, is so greatly dilated as to be liable to be mistaken for the ilium if found separately. All these parts are very nearly *in situ*, and the manner in which they unite to form the acetabular socket is easily perceived; the oval formation between the ischium and pubis is also quite distinct.

Humero-sternal parts.—In one of the specimens of Saurian remains, presented by Colonel Birch to the Museum at Oxford, the humero-sternal, or rather *humero-clavicular*, parts on one side of the animal are almost perfect. It is only at the extremities of the clavicle and scapula that the bones themselves are preserved; but the intermediate parts, though removed, have left an impression of their lower surface. Enough remains to enable us with certainty to identify these bones with more perfect specimens of the same, which have been found in a detached state. It is from these materials that I have effected the restoration of the humero-clavicular parts represented in Plate III. fig. 2.

The humero-clavicular parts consist, as in birds, and as in the lizard and some other reptiles, 1st, of coracoid bones separated from the scapula; 2d, of a small scapula; and 3d, of clavicles.

The coracoid bones in the specimen at Oxford are greatly elongated, in comparison of those represented in my first memoir, though resembling the latter in every other particular. I hesitate to consider this difference as specific, because the shorter coracoids evidently belonged to a much younger individual than the longer, as appears from the circumstance of these and other bones, which have become anchylosed in the latter case, remaining distinct in the former. I ought, however, to add, that a third fragment of this part, which certainly belonged to a large adult, and exhibits the anterior portion of the two coracoids adhering to a series of anterior dorsal vertebræ, agrees in form most nearly with the shorter specimen. The specimen belonging to the duke of Buckingham possessed the long coracoids, traces of them being very evident beneath all the anterior ribs. Should it appear on further

inquiry that there were two species, we learn from the specimens already procured that the specific distinctions were very slight, that noticed in the coracoids being in fact the only one that I have been able to detect after a careful collation of the most important parts in all the specimens that I have examined.

The scapula has been correctly represented in my first memoir; but the humerus, which I had there figured from the only specimen in which I had seen those two parts together (and which, having belonged to the late Mr. Catcott, is preserved in the public library at Bristol), in consequence of an accidental dislocation is exhibited in an inverted position. The clavicles consist of two transverse and one central piece. The former are the clavicles, strictly speaking; the latter may, perhaps, more properly be referred to the sternum. The corresponding part or furcular in the *Ichthyosaurus* also consists of two transverse and one central piece, as does that of the *Ornithorynchus*, when young, as has been noticed by Mr. Clift; but the central piece in these animals forms merely a short stem or handle (as it may be called) connected with the transverse clavicles, whereas in the *Plesiosaurus* it is considerably more developed. The general analogies between these parts in the reptile tribe, in the *Ornithorynchus*, and in birds, have been ably pointed out by Geoffroy St. Hilaire and Cuvier.

In the plate containing a restoration of the *Plesiosaurus*, (Plate III.) I have added, for the purpose of comparison, a sketch of this part in the *Ichthyosaurus*. That published in the Philosophical Transactions does not exhibit the tripartite division of the furcula, and erroneously makes its branches curve considerably too much upwards. The present outline is founded on three very perfect specimens, which entirely agree with one another in the parts here represented, and leave no doubt of their actual form.

Extremities.—The humerus articulates immediately with the bones, which in my preceding descriptions I had considered as the first row of the carpus; which contains only two instead of the three pieces placed together in the conjectural restoration. I have again to acknowledge the error into which I have been led in the insertion of a supposed radius and ulna between these parts; for the two pieces which form the first row formerly ascribed to the carpus, now appear to be the true representatives of the radius and ulna, though greatly differing in form from the usual type of those parts.

The conjectural restoration of the paddles would very nearly apply to the posterior paddles as exhibited in this specimen, by abstracting the outer bone from this supposed carpus, and removing

removing also the exterior and circular bones from the edges of the paddle as there drawn. I was led to introduce these exterior paddle-bones from the specimen represented, fig. 1, Pl. XLII. Geological Transactions, vol. v., in which they are so placed; but I have subsequently retracted this view, having learnt that when the specimen referred to was found, the bones in question were loose, and had been subsequently glued into their present situation, in consequence of a conjecture of the proprietor. I mention this circumstance lest any real inconsistency should be supposed to exist between that specimen and the more perfect and illustrative remains now discovered.

All the paddles are composed of two rows of nearly circular or discoidal bones, representing the carpus and tarsus, and of five digitated series, representing the metacarpal or metatarsal and phalangic bones, the distinction between these being inappreciable, though we may of course, in conformity to the usual nomenclature, term the first phalangic bones metacarpal, &c., if so inclined. The first or anterior digit on each paddle has four phalanges; the last, seven. These are evidently complete in the specimen. The whole five digits stand as follows:

Anterior paddle.		Posterior paddle.	
1st digit,	4 phalanges.	1st digit,	4 phalanges.
2d ...	7, and seems complete.	2d ...	8, complete.
3d ...	7, incomplete.	3d ...	10, uncertain whether complete or not.
4th ...	6, incomplete.	4th ...	9, uncertain whether complete or not.
5th ...	7, complete.	5th ...	7, complete.

This great multiplication of joints in the phalangic series strongly distinguishes this animal from all known quadrupeds. In the whole class of mammalia (some cetacea only excepted) the number of phalanges in the perfect and longest digits is restricted to three; it is the same in most of the reptiles; but some Saurians, *e.g.* the crocodile, exceed this number by one joint: birds also have only five phalanges.

A majority of the cetacea appear to possess only three phalanges, but in some species the number is increased; and the *Rorqual* (a species of *Balæna*) and the *Delphinus Delphis* present as many as seven (see Cuvier's *Ossemens Fossiles*, tom. v., and Camper's *Cetacea*, Pl. 44.), the nearest approximation to the number in the *Plesiosaurus*, though less than the number in the posterior paddle of that animal by two joints.

Although all the other analogies of the fossil animal refer it to a class widely differing from the cetacea, it is yet interesting to observe that in these instances, taken from beings of distinct general organization, the use for which nature in-

tended this part, viz. natation, being the same, a similar modification has been superinduced on the usual structure of quadrupedal extremities.

On the whole, this part in the *Plesiosaurus* presents a link between the usual structure and the still more complicated organization of the paddle in the *Ichthyosaurus;* the phalanges are flattened as in the turtle, and other animals destined for natation, and were doubtless in like manner covered by a common integument, and thus converted into a species of fins.

I shall conclude with some more general remarks.—In its motion this animal must have resembled the turtle more than any other; and the turtle also, as was before remarked, could we divest it of its shelly case, would present some slight approach in its general proportions to the *Plesiosaurus.*

I shall leave to others more conversant than myself with the analogies of comparative anatomy, the inferences to which those particulars may lead concerning the habits of this singular animal.

That it was aquatic is evident from the form of its paddles; that it was marine is almost equally so, from the remains with which it is universally associated; that it may have occasionally visited the shore, the resemblance of its extremities to those of the turtle may lead us to conjecture; its motion, however, must have been very awkward on land; its long neck must have impeded its progress through the water, presenting a striking contrast to the organization which so admirably fits the *Ichthyosaurus* to cut through the waves. May it not, therefore, be concluded (since, in addition to these circumstances, its respiration must have required frequent access of air), that it swam upon or near the surface, arching back its long neck like the swan, and occasionally darting it down at the fish which happened to float within its reach?

It may, perhaps, have lurked in shoal water along the coast, concealed among the sea-weed, and raising its nostrils to a level with the surface from a considerable depth, may have found a secure retreat from the assaults of dangerous enemies; while the length and flexibility of its neck may have compensated for the want of strength in its jaws and its incapacity for swift motion through the water, by the suddenness and agility of the attack which they enabled it to make on every animal fitted for its prey which came within its extensive sweep.

The name I have originally given to this animal, *Plesiosaurus* (approximate to the Saurians), may appear rather vague in this stage of our knowledge, and an appellation derived from its peculiar length of neck might be preferred; but for the present

sent I shall retain the old generic name, adding for specific distinction the well-known Homeric epithet *Dolichodeirus*, as characterizing the most striking peculiarity of its osteology. I am the rather induced to follow this course because I think it very probable, from specimens which I have examined, that species of *Plesiosaurus* with shorter necks exist in other strata. I have already figured a column, belonging to an animal of this genus, in which the proportions of the *Plesiosaurus Dolichodeirus* are inverted, the vertebræ of the neck being considerably thinner than those of the body. Professor Buckland has since obtained from Market Raisin, large fragments of the skeleton of the species to which that vertebral column must have belonged; its remains are common in the Kimmeridge or Oaktree clay. From its enormous size I shall provisionally indicate this species as *Plesiosaurus giganteus*, and I hope hereafter (in union with my friend) to submit drawings and a description of those remains to the Society.

With reference to the elucidation of all these questions, I cannot but congratulate the scientific public that the discovery of this animal has been made at the very moment when the illustrious Cuvier is engaged in, and on the eve of publishing, his researches on the fossil ovipara: from him the subject will derive all that lucid order which he never has yet failed to introduce into the most obscure and intricate departments of comparative anatomy.

LXVI. *Menstruum for Biting-in on Steel Plates.* By Mr. Edmund Turrell, *of Clarendon-street, Somerstown**.

THE demand that has taken place for engravings upon decarbonized steel plates, on account of their great durability when compared with copper plates, has caused many eminent artists to employ their talents upon that peculiar preparation of metal; and many beautiful specimens of line-engraving have been produced, capable of yielding proofs or prints to an extent unknown before the invention and application of that peculiar preparation of steel, which was first, I believe, made known to the world by Mr. Perkins, who has made use of it very extensively in his bank-note manufactory in the United States of America, and more recently in London.

If the execution of a fine engraving upon such prepared or decarbonized steel had depended entirely upon the graving tool, the principal difficulty that presents itself would be the superior

* From the Transactions of the Society of Arts.—The large gold medal was presented to Mr. Turrell for this communication.

superior hardness of the metal, which of course would offer greater resistance to the action of the graver than copper: but as most or all the engravings of the present time are a mixture of etching and graving united, it was of course equally necessary for the artist to be able to etch and bite-in upon decarbonized steel, as well as to cut with the graving tool.

In order to form a just idea of the difficulties that occur in etching and biting-in upon steel plates, it will be necessary to state a few facts relative to etching upon copper.

The usual method is to cover the copper plate with a coat of varnish, commonly called etching ground; and when the lines that are necessary to represent the subject are cut through the varnish with a point or needle, a border or rim of soft wax is raised round the sides of the plate, and nitrous acid, sufficiently diluted with water, is poured upon the whole surface, and immediately a corrosion of the copper takes place in those parts or lines where the varnish has been removed or cut through. The action of the acid is at the same time rendered obvious to sight by the continual formation and disengagement of bubbles of the nitrous gas on all the etched parts, thus indicating to the artist how the process is going on.

Various acids have been tried for this purpose, both singly and compounded in various proportions; but experience has proved that very pure nitrous acid is superior to any compound that has hitherto been produced, and I believe it is also superior to any other acid that can be used singly; for there is one requisite that is absolutely necessary and indispensable, namely, that whatever acid is used, it should not only have a powerful affinity for the copper, and by its chemical action corrode and deepen the etched lines, but it should also be capable of holding the oxide formed in perfect chemical solution, otherwise the lines would soon be choked up by a deposition of the oxide so formed; as the deposition increased, the oxide would press upon the edges of the etching varnish and loosen it, by which means a partial corrosion would take place under it, and shallow lines would be the consequence. The lines produced under such circumstances are also generally rough and uneven on their edges. The process just described is technically called *biting-in*, and such a production would be called a bad *biting*. On the contrary, when the oxide of copper formed during the process is immediately dissolved in the fluid that forms it, a fresh surface at the bottom of the line is continually offered to the acid to act upon, and then the corrosion produces the very best effect, that is to say, very deep lines, with beautiful clear and even edges.

When etching upon steel was first introduced, great difficulty

culty was experienced in biting-in the etched plates; for, whatever acid was used, it invariably happened that the lines when corroded were exceedingly shallow and rough upon the edges, many times so much so as to cause serious disappointments and great loss to those engaged. Such, indeed, was the risk of failure, that several artists have refused to execute any thing on steel, on account of the difficulty of biting-in their etchings.

I believe I am correct when I state that Messrs. Perkins and Heath paid the late Mr. Lowry fifty pounds for the secret of a menstruum that would effect the biting on steel in a manner superior to that which they had previously practised. It is but justice to state, that, previous to purchasing the before-named secret, their method was stated to all who applied to know it, and this consisted in using the worn-out acid that had been used for biting-in copper plates, which, therefore, was an acidulous nitrate of copper, in the state of solution. But with this the results were very unsatisfactory, and almost always the lines were so much shallower than those produced on copper, that the proofs from the plates in that state were very gray and spiritless, for want of depth to hold the proper quantity of ink.

No person was more sensible of this defect than the late Mr. Charles Warren, and the method of biting-in upon prepared steel, invented by him, and communicated to the public in the Transactions of the Society of Arts, &c. vol. xli. fully evince how warmly he entered into the investigation; and also, when he had attained a better method than was generally known, how liberally he presented it to his brother artists.

Had Mr. Warren stated in his communication that perfection was attained by this method, I should have conceived it an invidious task to dispute that point, by offering to the Society's attention a method of biting-in etchings, executed upon steel plates, which I feel confident has many advantages over any at present generally made known. But I am relieved from any such considerations, by the recollection of his expressions to the committee, that he not only came forward to contribute what he had invented, but was also very desirous to elicit facts and information, by leading others to make experiments on the subject, as he had done. I therefore trust that those who respect his memory, and feel grateful for his communication, will consider that I am following his example; and although I may not have found out a faultless improvement, yet that much new light will be cast upon the subject, tending to elicit new facts, and thereby bring to speedy maturity

turity a process that, in its present infant state, has many great and vexatious difficulties attached to it.

Shortly after Mr. Warren made his invention known to the Society, I was requested to execute some etchings on steel plates; but, previous to assenting, I thought it necessary to try how far the menstruum of Mr. Warren's invention would do for biting-in the even tints produced by machine-ruling, that being a kind of work more calculated to show the imperfection of bad biting than any other. And more particularly so when three lines are used to produce those aërial tints that are necessary to form the back-grounds to portraits, and to many other subjects, which, if produced by the graving tool, would (upon steel) be enormously expensive.

Having prepared the menstruum, according to the directions given, I certainly felt great difficulty in preventing a precipitation of the copper, which, filling the lines, continues to accumulate; and by its pressure, as it increases, removes the etching-varnish partially from the sides of the lines, and thereby causes, in a great degree, the shallowness before complained of.

I have no doubt, that on very small plates it may be possible to sweep the surface of the plate with such rapidity, that this evil may in a great degree be prevented; but upon large plates, where they are covered with work, such an operation will be attended with great difficulty, and in many instances will be nearly impossible.

In biting-in on copper plates, the breadth of the etched line is in a great degree indicated by the size of the bubble of nitrous gas formed on the line; but where the lines are filled or covered with the precipitated copper, the difficulty of judging of the state of the biting is greatly increased.

These and other difficulties incited me to give the subject every attention in my power. The first indispensable requisite (as it appeared to me) was to determine what acid would most readily corrode the lines etched upon the steel plate; and, after trying a number, nitric acid, reduced to a proper strength by dilution, appeared to me the best adapted to this purpose, provided some means could be found of preventing it from depositing the oxide of iron after having taken it up.

Chemists well know that iron exists in two states of oxidation, the protoxide and peroxide, and that each of these oxides will combine with acids forming two genera of ferruginous salts, the proto-salts and the per-salts. The proto-salts contain a larger proportion of oxide than the soluble per-salts; and being liable to pass into this latter state by long keeping,

or

or in a short time when exposed to the air, their solutions will sooner or later become turbid, and will deposit peroxide of iron in a state scarcely at all soluble, except by being digested in hot acid, combined with some deoxidating substance.

For this reason it is that the action of nitric acid diluted with water will seldom give satisfactory results when employed for biting-in; for although at first it acts very well, the iron being brought merely to the state of protoxide, and dissolving freely in the acid,—yet by exposure to the air, during the process of biting, it passes to the state of peroxide, a portion of which precipitates, and, falling into the lines of the etching, covers the surface of the steel at the bottom of these lines, and thereby impedes and renders irregular the action of the acid.

Knowing that calico-printers prefer making their solution of oxide of iron in pyroligneous acid, I conceived that, when in its pure state, this would be a very proper fluid to dilute the nitric acid with; because it would not only tend to reduce its action, but might prevent, or at least impede the precipitation of the oxide formed in the operation of biting. Although, however, something was gained by this addition, yet it did not appear to be so effectual upon repeated trials as to leave me completely satisfied It then came into my mind that alcohol, and, still more, ether, have a very powerful deoxidating effect, as they are able to separate gold, in its pure metallic state, from its solution in *aqua regia.* I determined, therefore, upon adding to the mixture of pyroligneous and nitric acids a portion of alcohol, expecting that the nitrous ether resulting from this combination being presented in its nascent state to the nitrate of iron formed during the biting, would retain it in the state of proto-nitrate, and thus prevent any precipitation. I am happy to say I was not in the least disappointed; for with a menstruum thus formed or compounded of the three ingredients, namely, pyroligneous acid, alcohol, and nitric acid, I acquire the following advantages.

In the first place, it corrodes the steel with great facility, producing a beautiful, clear, and deep line; and upon a variety of plates the results were very uniform.

Secondly, it prevents the deposition of peroxide; as a proof of which I have kept some of the menstruum which had been employed for biting-in as long as six months, and could not discover any precipitation formed.

As a further proof of its power of holding the oxide formed in perfect solution, it will be distinctly seen that as soon as the corrosion takes place upon the steel plate, the whole of the lines appear beautifully bright, and continue so until the biting-in is completed.

The proportions of the acids and the alcohol are as follows: Take four parts, *by measure*, of the strongest pyroligneous acid (chemically called acetic acid), and one part of alcohol, or highly-rectified spirits of wine; mix these together, and agitate them gently for about half a minute; then add one part of pure nitric acid; and when the whole are thoroughly mixed the menstruum is fit to be poured upon the etched steel plate.

When the menstruum is compounded in these proportions, very light tints will be sufficiently corroded in about one minute, or one minute and a half, and a considerable degree of colour will be produced in about a quarter of an hour. But the effect may be produced much quicker by the addition of more nitric acid, or it may be made to proceed much slower by omitting any convenient portion thereof.

The plate, when the mixture is poured off, should be instantly washed with a compound made by adding one part of alcohol to four parts of water; and the best material for stopping out any part that is sufficiently corroded is pure asphaltum dissolved in essential oil of turpentine, which of course must be of sufficient consistence to flow freely from a hair pencil. It may be necessary to inform those engravers that use the common Brunswick-black to stop out the bitings on copper plates, that it is a very improper article to use on steel plates; because, as the asphaltum and oil of turpentine, of which it is principally composed, do not render it sufficiently drying, these ingredients are digested with a small quantity of spirit of wine, which has a great tendency to unite with the biting menstruum above described, and thereby cause foul biting.

As I attach considerable importance to the purity of the ingredients of which my menstruum is composed, I beg leave to impress on my brother artists the necessity of strict attention to this circumstance: I have myself obtained the ingredients in a state entirely to my satisfaction from Mr. Desormeaux, 16, Charlton-street, Somerstown.

LXVII. *On the Invention, Progress, and Advantages of the Art of Engraving in Mezzotinto upon Steel. By Mr.* Charles Turner, *of Warner-street, Fitzroy-square**.

THE discovery of a method of engraving in mezzotinto upon steel may be justly regarded as one of the most fortunate occurrences in the history of the graphic arts. In the infancy

* From the Transactions of the Society of Arts.

of

of this invention, as in that of almost every other, difficulties repeatedly presented themselves, but these have been successfully overcome; and the art, an outline of the gradual advance of which I am about to subjoin, may at present be considered as arrived at a state of full and vigorous maturity.

In the year 1812, that distinguished ornament and benefactor of his country, the late Mr. James Watt, suggested to me the possibility of engraving in mezzotinto upon steel; but all the attempts with which I immediately followed up the communication were unsuccessful. The hardness of the steel induced me to lay that metal wholly aside; and some experiments which I subsequently made upon plates of brass were attended with no better results: these latter substances were so unequal in their temper, that I found them quite unfit for my purpose.

It was not, therefore, till the very recent date when Mr. Jacob Perkins (whose indefatigable labours and extraordinary inventive powers are so well known and so highly appreciated) had produced blocks of steel soft enough to receive the impressions of our tools, and form a ground upon which we have been able to accomplish every thing required, that the art of engraving in mezzotinto upon steel can be said to have had its commencement.

In the month of January 1820, Mr. Say made an engraving on one of Mr. Perkins's blocks, and it was decidedly the best specimen then produced. In February 1821, I engraved a portrait on the first steel plate I ever saw. It had been given to me by the late Mr. Lowry, and it turned out so satisfactorily as to meet the approbation of Sir Thomas Lawrence.

On the 30th of May 1822, Mr. Lupton received the gold medal from your Society for his admirable performance,—the Infant Samuel. From the good fortune which has crowned the latter experiments, steel plates have obtained a decided preference over those of copper for engraving in mezzotinto; and the beautiful specimens now before the public, from the hands of Messrs. Ward, Reynolds, Say, Lupton, and other artists, require only to be seen to be admired.

In engravings in mezzotinto upon steel the tones are far better defined than those obtainable upon copper. From the superior density of the former metal, the clearness of the lighter tints is carried to much greater perfection; and, from the same cause, the darks have also a decided preference, being distinguished by their superior richness. The advantages in these respects are so numerous, that all the deficiencies, which were formerly irremediable in mezzotinto engraving, are now entirely mastered; and the numerous difficulties with which the artist was always contending, completely dissipated: although

the process is much longer and more tedious on steel than on copper; yet, when completed, it is so perfectly satisfactory as fully to reward the additional labour. The instruments used in engraving in mezzotinto upon steel are precisely the same as those employed in engraving upon copper. When a deep black is required, twice the number of ways will be found desirable; from sixty to a hundred will not be too many. Steel plates are now so well prepared, and are become so common, that they are easily obtained by all who desire them; the best are those manufactured by Mr. Rhodes, and Mr. Hoole, of Sheffield, who have paid every attention to rendering them fit for the purpose of the artist: they may also be had of Mr. Harris, Shoe-lane, London.

I believe that I shall not be thought to entertain an erroneous opinion, when I express my belief that it is solely to the introduction of engraving upon steel into this country by Mr. Perkins, that we are indebted for the present successful application of the same metal to the art of engraving in mezzotinto.

In conclusion, it may be serviceable to add a warning and receipt, which the novelty of the use of steel in the art of engraving somewhat imperiously requires:—Great care should be taken to save the steel from rust, which is done by warming the plate, and rubbing sheep's suet (from the animal) over it, and keeping it near a fire, or in a dry room; without this precaution much mischief will arise.

LXVIII. *Observations on the dichotomous Distribution of Animals: together with a Binary Arrangement of the Natural Order* Saxifrageæ. *By* A. H. Haworth, *Esq. F.L.S. &c.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

SINCE my last communication to you, I have seen for the first time (viz. June 3, 1825) a work entitled *Zoologie Analitique*, by A. M. Constant Dumeril, printed at Paris in 1806. It was lent to me by my friend Mr. H. Boys, one of the Fellows of the Linnæan Society; and it distributes the genera of animals in a method so allied to my own binary one, that it may, perhaps, be said by some, that I have borrowed largely from it without any acknowledgement. Wherefore it becomes necessary that I should assure the readers of your Magazine to the contrary; and that it was in consequence of my explaining what I thought the complete novelty of *my* plan to Mr. Boys, that he showed and lent me the ingenious and elaborate work of Dumeril, as above stated. But I have not yet seen the botanical publications which that author mentions in his preface, although

although I possess Hooker and Taylor's *Muscologia Britannica*, which affords a somewhat similar plan, as appears by their preface, though I have not followed it.

The difference between the method of Dumeril and my binary one may be seen by the following remarks. His divisions are not invariably worked out through dichotomies, but are sometimes trichotomous, tetrachotomous, or in one or more instances forkless.

His method is essentially *descriptive* through every branch, and without names, until you arrive at a genus. Mine is *definitive*; and every division, subdivision, &c., is appropriately designated by a single word, which is its *name*.

His dichotomies bring you down to every individual genus, yet the genera are often *forced* into dislocations, as their irregular numerical figures confess; but which the accompanying page-work corrects. My tables, or rather their dichotomies, usually end in two or more genera located *naturally*, and which are intended to be numbered currently; and the page-work which I contemplate, and its head-lines, will have corresponding numbers, conducting the reader instantly to characters, descriptions, and synonyms, of every published genus.

The essence, however, of both Dumeril's and my own plan is, that they equally work, or ought to work, *ever dichotomously*, and by *opposites*. In a word, mine at least is intended to be *polaric*; and the very title of the book which I meditate is, *Organica Corpora, methodo polari digesta*. Wherefore, if we can admit that the dichotomies of the tables throw their component subjects as distant as possible in point of affinity from each other (being frequently merely analogies), and surely this is possible, and will ultimately be practicable,—it clearly follows, that every thing which lies, in point of affinity, between such groups, must absolutely, through the constantly repetite spirit of the arrangement, fall *between* them; and therefore fall naturally, and truly, in the *very* place where it *ought* to fall. And from this it results, as I have elsewhere observed, that the apparent distribution by Nature of her organized forms, is a *continuous* one that is unravelable in the way of a straight line; but arising dichotomously from one root (which I have called MATTER), and proceeding in a repetitely double, and in point of magnitude or quantity, unequal series; resembling, as it were, an inverted branching and exuberant tree. And I have also elsewhere observed, that every dichotomy extending into, and ending in, various genera, may be imagined (by bending its ends together) as forming a sort of circle of affinity, either open at top, or closed by the root which produces it. These minor circles, moreover, are but parts of other

other larger circles, which the *mind's eye* may imagine to be composed of several, one within another; or, when taking the widest possible range, the whole may be conceived to be but or reiteratedly compounded circle, vast and grand, enfolding or representing in its *ample orb* the mighty works of God on earth. I have the honour to remain, sir, yours, &c.

Queen's Elm, Chelsea, June 5, 1825. A. H. HAWORTH.

Postscript.—It may be worth adding, that the first time I ever observed the prevalence of a dichotomous distribution in nature, was in the winter of the year 1813–14, when engaged in arranging and grouping the genera for my *Saxifrageärum Enumeratio*; which, although shown soon after to some friends, was not published until 1821. The distribution of the genera there given is virtually and in spirit a dichotomous one (though I did not then venture to print it so); and this will appear by the conciseness and precision of the following table, which really alters not either the natural or current location of a single genus there published.—Vide *Saxifrag. Enum.* p. 4. —The genera are in italics below.

SAXIFRAGEÄRUM TABULA DICHOTOMA.

SAXIFRAGEÆ.

- UNIVALVES.
  - Rectocalycatæ.
    - Acaules.—*Megasea, Dermasea, Chondrosea.*
    - Foliosæ.— *Miscopetalum, Lobaria, Tridactylites, Saxifraga, Muscaria, Leptasea, Hirculus, Ciliaria, Antiphylla.*
  - Reflexocalycatæ.
    - Sessilifoliæ*. - *Micranthes...*
    - Petiolatæ.
      - Decurrentes.—*Aulaxis, Spatularia.*
      - Edecurrentes.
        - Sarmentosæ.—*Ligularia...*
        - Esarmentosæ.—*Robertsonia...*
- BIVALVES.
  - Decandræ.—*Mitella, Tiarella.*
  - 8—5-andræ.
    - Uniloculares.—*Chrysosplenium, Adoxa.*
    - Biloculares.—*Heuchera...*

LXIX. *Description of an improved Cross for Land-Surveyors. By Mr.* ISAAC NEWTON.

To the Editor of the Philosophical Magazine and Journal.

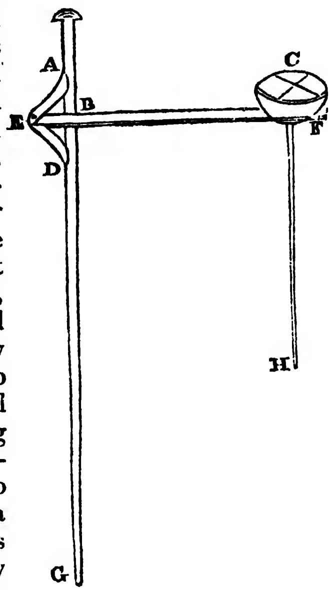
Sir,

IT being generally allowed and experienced by land-surveyors that the measuring of diagonals, perpendiculars, &c., by means of the chain and the common cross, is attended with a good

* Misprinted "Sessilifloræ" in *Saxifrag. Enum.* p. 4, &c. but not in p. 45.

good deal of labour, I hope the following description of an improved cross, lately invented by myself, as it is calculated to abridge in a great measure the labour here complained of, will not be found uninteresting to some of the readers of the Philosophical Magazine.

A B D G represents the upright staff, and C the cross fixed at the upper end of a slight straight rod of wood F H, and resting on an horizontal arm or flat and straight piece of wood E B F, having two circular holes at B and F, large enough through which to admit the staff and rod at right angles to the arm; which arm is moveable about the staff, and may revolve about it at pleasure. A E D is a small spring or bent piece of iron, capping the arm at E, where, being riveted, it presses on the staff at A and D, above and below the arm, and serves to adjust the arm to any height on the staff, and to keep the arm in any required horizontal direction. The rod F H being fixed to the cross at F, and parallel to the staff, and also free to turn in the hole at F, answers a threefold use; as it keeps the cross on the arm, turns the sights in any direction, and points out where, on the ground, the chain must fall.



In practice I think it will be found most convenient to have the radius or part of the arm B F about 18 inches long, and the rod F H about 3 feet and a half in length.

In measuring diagonals, &c. by means of the chain and *common* cross, the young practitioner is frequently obliged to prick his staff in six or seven different places before he can succeed in finding the diagonal, &c.; but in using the above, he will seldom find it requisite to ground his staff more than once, in order to answer the same purpose. For instance, having brought his cross as near to the diagonal, &c. as he can guess, the error, if any there be, may easily be corrected by the radius B F, which, as before observed, is made to revolve about the staff. I remain, sir, yours, &c.

Wisbech, June 8, 1825. ISAAC NEWTON.

P. S.

P.S. The turner who makes the staff A B D G may leave it capped at top, and make it cylindrical about half its length from the top, in order that the arm may not slide down the staff when it ought to remain fixed.

LXX. *On Mr.* J. Herapath's *Demonstration of the Binomial Theorem. By Mr.* L. T. Ward.

To the Editor of the Philosophical Magazine and Journal.

Sir,

At page 323 of the Philosophical Magazine for last month, Mr. J. Herapath, in his demonstration of the Binomial Theorem for integral exponents, tells us that the product of $(q-1)$ of the quotients $n, \frac{n-1}{2}, \frac{n-2}{2}$, &c., is equal to the co-efficient of the qth term in the expansion of $(x+y)^n$ when q, of course, is any whole number greater than 2. This of itself amounts to no proof. In order to complete the demonstration it must be shown that $n \times \frac{n-1}{2} \times \frac{n-2}{2}$, &c. to $(q-1)$ factors, OUGHT to equal the co-efficient of the qth term in the expansion of $(x+y)^n$, and this may easily be done by induction from the operations marked B.

Thus $2 \times \frac{1}{2}$

$$3 \times \tfrac{2}{2};\ 3 \times \tfrac{2}{2} \times \tfrac{1}{3}.$$

$$4 \times \tfrac{3}{2};\ 4 \times \tfrac{3}{2} \times \tfrac{2}{3};\ 4 \times \tfrac{3}{2} \times \tfrac{2}{3} \times \tfrac{1}{4}.$$

$$5 \times \tfrac{4}{2};\ 5 \times \tfrac{4}{2} \times \tfrac{3}{3};\ 5 \times \tfrac{4}{2} \times \tfrac{3}{3} \times \tfrac{2}{4};\ 5 \times \tfrac{4}{2} \times \tfrac{3}{3} \times \tfrac{2}{4} \times \tfrac{1}{5}.$$

&c. &c. &c.

$$n \times \frac{n-1}{2};\ n \times \frac{n-1}{2} \times \frac{n-2}{3};\ n \times \frac{n-1}{2} \times \frac{n-2}{3} \times \frac{n-3}{4}$$

$$\left(n \times \frac{n-1}{2} \times \frac{n-2}{3} \ \ldots\ \frac{1}{n}\right)$$

It is easy to perceive how these results are obtained; and on comparing them with the co-efficients of the powers marked A, it is evident that $2 \times \frac{1}{2}$ = the co-efficient of the 3d term of the 2d power; that $3 \times \frac{2}{2}$ and $3 \times \frac{2}{2} \times \frac{1}{3}$ = the co-efficients of the 3rd and 4th terms of the 3rd power; and so on of the rest; and therefore, that $n \times \frac{n-1}{2}$; $n \times \frac{n-1}{2} \times \frac{n-2}{3}$; $n \times \frac{n-1}{2} \times \frac{n-2}{3} \times \frac{n-3}{4}$, &c., which are deducible from the preceding steps, properly denote the co-efficients of the 3rd, 4th, 5th, &c. terms in the expansion of $(x+y)^n$.

I am, sir, yours, &c., L. T. Ward.

Wisbech, June 6, 1825.

LXXI. *A*

LXXI. *A Description of a new Species of* Scolopax *lately discovered in the British Islands: with Observations on the* Anas glocitans *of Pallas, and a Description of the Female of that Species. By* N. A. Vigors, *Esq. A.M.F.L.S.**

THE Natural History of these islands has been studied with so much science and assiduity, and the investigation attended with so much success, that any addition to the number of our species, more particularly in the higher classes of the Vertebrated Animals, must be of rare occurrence, and productive of considerable interest. It is therefore with much pleasure that I submit to this Society the following description of a species of *Scolopax*, new to the Ornithology of the British Islands; and of a species of the Linnæan genus *Anas*, which, from its having been until recently but once only observed in this country, and from the specimen which was described not being at present in existence, or capable of being referred to, has been for some time considered as possessing but a doubtful claim to a place in the British Fauna.

Ordo. Grallatores, Ill.

Fam. Scolopacidæ, mihi.

Genus. SCOLOPAX, Auct.

Scolopax Sabini.

S. castaneo atroque varia, subtus pallidior, pileo humeris pteromatibus remigibusque atris, rostro pedibusque fusco-atris.

Rostrum fusco-atrum, *mandibulâ* superiore ad basin subcastaneâ. *Gula, genæ, pectus*que fusco-atræ, castaneo-maculatæ. *Abdomen* fusco-atrum castaneo-fasciatum. *Tectrices* inferiores, *remiges*que subtus fuscæ. *Dorsum scapulares*que intensè atræ castaneo-fasciatæ. *Rectrices* duodecim, ad basin atræ, ad apicem ferrugineæ atro-fasciatæ.

Longitudo *corporis*, rostro incluso, $9\frac{3}{10}$; *rostri* $2\frac{7}{10}$; *alæ* a carpo ad remigem secundam $5\frac{1}{10}$; *tarsi* $1\frac{1}{4}$.

In Mus. nos.

This species is at once distinguished from every other European species of *Scolopax*, by the total absence of white from its plumage, or any of those lighter tints of ferruginous-yellow, which extend more or less in stripes along the head and back of them all. In this respect it exhibits a strong resemblance to the *S. saturata* of Dr. Horsfield, from which however it sufficiently differs in its general proportions: and I find no description of any other extra European species of true *Scolopax*

* From the Transactions of the Linnæan Society, vol. xiv.—Communicated by the Zoological Club of the Linnæan Society.

 which

which at all approaches it in this character of its plumage. In the number of the tail-feathers again, which amount to twelve, it differs from *S. major*, which has sixteen, and *S. gallinago*, which has fourteen: it agrees however in this point with *S. gallinula*, which also has but twelve; but it never can be confounded with that bird, from the great disproportion between the essential characters of both; the bill alone of *S. Sabini* exceeding that of the latter species by one-third of its length. In the relative length and strength of the *tarsi* it equally differs from all. These members, although stouter than those of *S. gallinago*, fall short of them by $\frac{3}{20}$ of an inch: they are much weaker, on the other hand, than those of *S. major*, although they nearly equal them in length. In general appearance it bears a greater resemblance to *S. rusticola* than to the other European *Scolopaces*, but it may immediately be recognised as belonging to a different station in the genus; the two exterior toes being united at the base for a short distance, as in the greater number of the congeneric species; while those of *S. rusticola* are divided to the origin.

The only specimen of this species with which I am acquainted, the description of which is accompanied by a very accurate drawing* by Mr. Curtis, is the bird in my possession. It was shot in the Queen's County, in Ireland, by the Rev. Charles Doyne, of Portarlington, in that county, on the 21st of August 1822; and was obligingly communicated to me the same day. I have named the species in honour of the Chairman of the Zoological Club of the Linnæan Society, whose zeal and ability have thrown so much light upon the Ornithology of the British Islands†.

Ordo. NATATORES, Ill.

Fam. ANATIDÆ, Leach.

Genus. QUERQUEDULA, Briss.

QUERQUEDULA GLOCITANS.

Q. fusca nigro-undata, capite viridi supra nigro subcristato; maculâ ante poneque oculos ferrugineâ, pectore ferrugineo

* A figure is given in the Linn. Trans. vol. xiv. Tab. XXI.

† Since the above communication was read to the Society, I have been enabled to record a second instance of this bird having been met with in the British Islands. On the 26th of October 1824, a female of this species was shot on the banks of the Medway, near Rochester, and is preserved in the valuable collection of Mr. Dunning of Maidstone. The specimen was kindly communicated to me by that gentleman, and was exhibited to the Zoological Club on the 23rd of November 1824. It accords in every particular with the specimen above described, with the exception of being somewhat smaller. This difference of size most probably indicates the difference of sex.

maculis

maculis nigris, tectricibus duabus mediis lateralibus longioribus.

Anas glocitans. *Pallas, Acta Stock.* 1779. xl. *t.* 33. *f.* 1.

——————— *Gmel. Syst.* i. *p.* 526.

——————— *Lath. Ind. Orn. p.* 862.

Bimaculated Duck. *Pennant, Brit. Zool. vol.* ii. *p.* 602. *t.* 100. *f.* 2. *ed.* 1776.

Mas. *Rostrum* plumbeum, dertro nigro. *Pileus* niger ferrugineo-varius. *Genæ collique* latera virides. *Guttur* viridi-nigrum. *Pectus abdomenque* medium ferruginea, maculis nigris, superioribus rotundis, inferioribus ovalibus. *Dorsum abdominisque* latera fusca lineis nigris gracilibus undata. *Scapulares* nigro-undatæ, ad apicem nigræ. *Ptila, pteromataque* superiora fusca, his fasciâ latâ ferrugineâ apicali marginatis; inferiora alba. *Remiges* fuscæ; speculo violaceo-viridi, fasciâ mediâ nigrâ, apicali albâ. *Uropygium, caudæque tectrices* viridi-nigræ. *Rectrices* fuscæ, albido-marginatæ, duabus mediis nigris, laterales longitudine excedentibus. *Pedes* lutei, membrano in medio nigro.

Longitudo *corporis*, rostro incluso, $15\frac{3}{4}$; *rostri* ad frontem $1\frac{9}{10}$, ad rictum $2\frac{1}{10}$; *alæ* a carpo ad remigem secundam $8\frac{2}{5}$; *tarsi* $1\frac{1}{2}$.

Fœm. *Rostrum* plumbeum dertro fusco. *Caput gutturque* albidè ferruginea, isto nigro-lineato, hoc parcè nigrosparso. *Pectus, dorsum, uropygium, abdominisque* latera fusca ferrugineo-marginata. *Abdomen* subtus album. *Rectrices* mediæ fuscæ, lateralibus haud longiores. *Alæ, pedesque* ut in mare.

Longitudo *corporis*, rostro incluso, $15\frac{1}{2}$; *rostri* ad frontem $1\frac{7}{10}$, ad rictum 2; *alæ* a carpo ad remigem secundam $8\frac{1}{5}$; *tarsi* $1\frac{9}{20}$.

In Mus. nost.

The male of this species was first described by Mr. Pennant in his British Zoology, under the name of *Bimaculated Duck*, and introduced as an inhabitant of the British Islands in the following words:—"Taken in a decoy in 1771, and communicated to me by —— Poore, Esq. *" The same bird was afterwards described and figured by Dr. Pallas in the *Acta Stockholmiensia* for 1779 as a native of Siberia, frequenting lake Baikal and the river Lena; and was named by him *Anas glocitans*. On the authority of Mr. Pennant † the species has

* Linn. Trans. vol. ii. p. 603.

† I take Mr. Pennant's authority (see Arctic Zoology, p. 575,) for determining that his *Bimaculated Duck* and the *Anas glocitans* of Dr. Pallas are the same species. From the figure given in the *Acta Stockholmiensia*, I could scarcely

has subsequently been included among the Birds of Great Britain by writers on British Ornithology; but no further account has reached us of the specimen alluded to by that distinguished naturalist, nor has it been ascertained whether it was preserved after it was communicated to him. The specimens of both male and female, from which I have taken the above description, were sent up from a decoy near Maldon in Essex, to Leadenhall-market, in the winter of 1812-13. Here they were observed by a respectable naturalist *, who immediately purchased them and set them up. From his collection they have subsequently passed into mine. There can be little doubt of the two birds being sexes of the same species. They agree in all the essential particulars that serve to identify the species of this family; their bill, legs, and feet exactly according in structure, and the colouring and markings of the *speculum* on the wings, a distinguishing character among the *Anatidæ*, being precisely the same. We have moreover, in favour of this conclusion, the negative evidence that the other sex of neither of these birds has until now been ascertained; and we have the positive evidence that both these specimens were taken in the same decoy and at the same time.

The *Querquedula glocitans*, or *Bimaculated Duck*, is readily distinguished from every other species of the family by the two ferruginous spots on the cheeks, in conjunction with the form of its tail, in which the two middle feathers somewhat exceed the others in length. The other European species of the *Anatidæ*, whose tails are elongated, are the *Anas glacialis*, *A. boschas*, and the *A. acuta* of Linnæus †. From the former of these it is at once distinguished by strong generic characters; the *A. glacialis* ‡, from its lobated *hallux*, its legs being thrown behind the equilibrium of the body, and its consequently superior habits of swimming and diving, being placed at that extreme end of the family which leads off to the true oceanic birds, or typical *Natatores;* while the *Q. glocitans* be-

scarcely myself draw that conclusion; the round spots on the side of the head in the former species being superseded by long narrow stripes in the figure of the latter; while the tail is completely rounded, the two middle feathers not being longer than the rest. Mr. Pennant's own figure of this bird is an excellent representation. I must here notice what appears to be a slight difference between our two birds. In the British Zoology the species is described as having twelve tail-feathers: in my specimens, both of male and female, there are sixteen.

* Mr. George Weighton, of Fountain Place, City-road.

† These are the *Harelda glacialis*, *Anas boschas*, and *Dafila acuta* of Shaw's Zoology.

‡ The *Anas nigra*, Linn. also has the tail somewhat acute; but, equally with *A glacialis*, stands at a remote extreme of the family from *Q. glocitans*.

longs

longs to those groups, which, with a free *hallux*, legs placed within the equilibrium of the body, and inferior powers of swimming and none of diving, affect the neighbourhood of fresh waters, feed occasionally on land, and as such form part of the aberrant subdivisions of the Natatorial Order. It is evidently remote from *A. boschas*, of which the middle tail-feathers also appear the longest, but which are invariably curved upwards. While it may also be perceived to hold a different station from *A. acuta*, which, although closely allied to the same group, yet from its long neck and legs is found to stand at that remote end of it where it is connected with the *Anseres*, the next conterminous division of the family. Its nearest affinity among the European species is to the *A. circia*, Gmel., and *A. crecca*, Linn.*

The appearance of this species in the British Islands seems of rare occurrence; two instances only of the kind having been recorded. These most probably are to be attributed to some extraordinary accident or stress of weather.

LXXII. *An experimental Inquiry into the Nature of the radiant heating Effects from terrestrial Sources.* By Baden Powell, *M.A. F.R.S., of Oriel College, Oxford.*†

(1.) THE nature of the heating effect emanating from *luminous* hot bodies has been distinctly shown to be, in many particulars, very different from that evolved from *non-luminous* sources; but the ideas commonly entertained on the subject are far from being precise and distinct. To gain, if possible, some ground for establishing more clear views, is the object of the following inquiries.

(2.) Professor Leslie, in his well known and elegant experiments (Inquiry concerning Heat, &c., chap. iii.) has fully established the theory of the effect of screens on radiant heat; and these effects give some of the most important criteria for examining the nature of radiating agents.

Those experiments apply only to the heat evolved from a non-luminous source. It therefore naturally becomes the subject in question, Whether the interceptive power of glass is not limited to a certain temperature, or state, of the radiating source: and to this point accordingly the attention of several eminent observers has been directed in many well known investigations, among which those of M. De La Roche are justly regarded as the most important and complete. In these ex-

* The *Querquedula circia* and *Q. crecca* of modern ornithologists.
† From the Philosophical Transactions for 1825. Part I.

periments

periments it appears, that a greater effect is produced on a blackened thermometer when a glass screen is interposed, in proportion as the body under trial approaches nearer its point of luminosity, or becomes more intensely luminous. (Biot, *Traité de Phys.* tom. iv. p. 638. Ann. of Philos. O.S. vol. ii. p. 163.)

Both M. De La Roche and M. Biot (see Biot, iv. 612) seem disposed to view the results obtained by the former, upon the supposition of one simple agent, the principle both of light and heat. This is at first radiated as heat; at a certain point it begins to assume the form of light, when the interceptive power of glass decreases in proportion to the increase of luminosity.

(3.) As long as the hot body continues below the temperature of luminosity, the partial or total interception of the effect is precisely the same phænomenon as that described by Professor Leslie in his experiments on screens, and explicable in the same way. (Phil. Trans. 1816, Part I. "On new Properties of Heat," Prop. 40.) And the apparent transmission of a portion of the effect must be referred to the same principle, as is clearly shown by Dr. Brewster, who has established, apparently beyond contradiction, the impermeability of glass to simple radiant heat upon quite independent principles.

(4.) Above the temperature of luminosity we must have recourse to further considerations. The hypothesis of MM. De La Roche and Biot appears to be nearly the same as that of Professor Leslie. (Inquiry, p. 162.) And it certainly has the merit of simplicity and satisfactory explanation of the phænomena. But it is an opinion which has not received direct proof; and it is also obvious that the phænomena may be explained without it; for we may just as well account for the facts, by supposing two distinct heating influences, one associated in some very close way with the rays of light, carried as it were by them through a glass screen without heating it; the other being merely simple radiant heat, affected by the screen exactly as the radiant heat from a non-luminous body.

(5.) In order to ascertain which of these suppositions is true, it will not be sufficient to observe the effects produced by the intervention of a *screen alone.* We must combine this method with an examination of the relations of different sorts of heat to *surfaces.* These relations have been shown to differ according as the body is luminous, or not: in the one case, the direct heat affects bodies in proportion to the darkness of their *colour*, without regard to the texture of their surface; in the other, the magnitude of the effect depends solely on the *absorptive texture*, without reference to colour. I use the term "absorptive

"absorptive texture," to signify that peculiar state of division in the particles of the surface, which has been shown by Professor Leslie and others to be most susceptible of the influence of simple radiant heat, and always to give a proportionally greater radiating power.

The question then is entirely one of facts, and involves no hypothesis as to the nature either of light or of heat. The object is simply to ascertain by experiment, whether, of the total heating effect radiated from a luminous hot body, the portion intercepted by a transparent screen is of the same nature as, or different from, the part transmitted in its relations to the surfaces on which it acts.

(6.) In conformity with this view of the object proposed, the general principle of the following experiments is this: taking different luminous hot bodies, to expose to their influence two thermometers, presenting one, a smooth black surface, the other an absorptive white one; thus obtaining the ratio of the total direct effect on the two, we may compare it with the ratio similarly observed when a transparent screen is interposed.

(7). This principle of experimenting was applied with one or two variations; and though in the abstract sufficiently simple, it will in practice require an attention to several considerations. I shall therefore proceed in the first instance to the detail of the different particulars; then give the results of the experiments in a tabular form; and lastly, recapitulate the conclusions and make a few general remarks.

I. (8.) In the following set of experiments two common thermometers were employed. The diameters of their bulbs were, thermometer A, 0·6 inch.; B 0·55. A was coated with a wash of chalk and water, and B with Indian ink.

In order to compare the effects to be observed with those of simple radiant heat, I ascertained the ratio of the effects of the latter on the two bulbs thus coated, by a few preliminary trials, and found it to be very nearly one of equality, or perhaps, the effect of the white rather greater than that of the black.

The two thermometers were graduated to quarters of centigrade degrees; and were both fixed on one mounting, with their bulbs detached about one inch from its lowest part, and at the distance of about three quarters of an inch from each other.

(9.) In the 2d set of experiments they were fixed into the top of a box, the front of which was open, so that the glass screen could be applied to it or not, as required. When the screen was not used the box would acquire more heat, and radiate it to the bulbs in a small degree; which affecting them in

in the inverse ratio of their diameters, would diminish the ratio of their risings. That this diminution was very trifling, and not at all sufficient to account for the observed difference of ratio will be evident, because the 1st set was made without employing the box, the thermometers being suspended at a distance from any object which could radiate heat to them; and in this set the difference of ratio is quite as conspicuous. This remark applies likewise to the possible communication of heat by the air.

(10.) We must also take into consideration the effect due to the glass screen. When we consider the two bulbs as heated only by that part of the radiation which is transmitted through the screen, the screen may be regarded simply as a third body placed near the two bulbs; and whether it possesses a higher or a lower temperature, there will be a tendency to bring all three to an equality in proportion to the difference of temperature, and in the bulbs, dependent on their diameters modified by the state of their surfaces. This effect arises from simple radiant heat; whilst that derived from the luminous hot body, is evidently following a different law with regard to the surfaces. It will easily follow, from what has been already shown, that such a secondary heating effect will be of a kind tending to *diminish* the ratio otherwise obtaining between the effects on the two bulbs. If the effect were of a cooling nature, the same thing would also take place; for I ascertained that the radiating powers of the coatings employed, deduced from the observed rates of cooling, were in a ratio which happened to be almost exactly the inverse of that of the diameters; but this effect is probably always small; and I have roughly allowed for it, as will be seen immediately; taking the temperature of the screen by a small thermometer having its bulb in contact with the central part of the surface.

II. (11.) I now proceed to state the results, which will be most conveniently exhibited in a tabular form.

1st Set. Incandescent iron. Distance 7 inches.

Glass Screen.

Experiment.	Rise of Thermometer in 1 min. centigrade.	
	A. white	B. black.
1	1·25	2·5
2	1·25	3·
Mean	1·25	2·75
Allowing for the screen as below	1·	2·5

No

No Screen.

Experiment.	Rise Therm. 1 min. cent. A. white.	B. black.
1	7·5	9·75
2	6·5	7·75
Mean	7·	8·75
Difference of exposed and screened results	6·	6·25

(12.) Argand lamp without its chimney. Distance 3 inches.

Glass Screen.

Experiment.	Rise of Thermometers in 1 min. centigrade. A. white.	B. black.
1	·75	1·75
2	·5	2·25
3	·75	2·25
Mean	·66	2·08
Allowing for the screen . . .	·41	1·83

No Screen.

1	1·75	3·5
2	1·75	·3·25
3	2·	3·5
Mean	1·83	3·41
Difference of exposed and screened results	1·42	1·58

(13). 2nd Set. Incandescent iron. Distance 6 inches.

Glass screen 2 inches from bulbs.

Experimt	Temperature of Screen before Experiment by Thermometer in contact.	Rise of Thermometers in 1 min. centigrade. A. white.	B. black.	Temperature of Screen after Experiment.
1	16·5	1·5	1·5	25·5
2	16·5	·5	1·25	23·75
3	17·	·5	1·5	24·5
4	17·	·5	1·	22·25
5	17·	·5	1·	22·25
Mean		·6	1·25	
Effect of the Screen alone, heated above 25°.				
1		·25	·25	
2		·25	·25	
The former result diminished for this effect		·35	1·	

Incandescent iron. No Screen.

Experimt	Temperature of Screen before Experiment by Thermometer in contact.	Rise of Thermometers in 1 min. centigrade. A. white.	B. black.	Temperature of Screen after Experiment.
1		3·	3·5	
2		3·	4·	
3		2·75	3·5	
4		3·3	3·75	
5		3·3	4·	
Mean		2·95	3·75	
Difference of the exposed and screened results		2·6	2·75	

(14.) Flame of an Argand lamp without its chimney. Distance 3 inches.

Glass Screen 1·5 inch from bulbs.

Experimt	Temperature of Screen before Experiment.	Rise of Thermometers in 1 min. centigrade. A. white.	B. black.	Temperature of Screen after Experiment.
1	17	1·25	2·25	23
2		1·25	2·25	
3		1·75	2·25	
4		1·25	2·75	
5		1·	2·25	
Mean		1·3	2·35	
Effect of the Screen alone, heated above 25°.				
		·25	·25	
The former result diminished from this effect		1·05	2·10	

Lamp. No Screen.

1		2·25	3·	
2		2·5	3·25	
3		2·25	3·	
4		2·25	3·25	
5		2·5	3·5	
Mean		2·35	3·2	
Difference of the exposed and screened results .		1·3	1·1	

(15.) In

(15.) In these experiments it will be evident upon inspection that the ratio of the effects produced on the white and black bulbs is in every instance considerably greater, when they were affected only by that part of the total heating influence which is transmitted through a transparent screen, than when they were exposed to the whole. This then would indicate, that on the removal of the screen some new heating power was brought into action, which affected the ratio by the addition to each of its terms, of quantities in a ratio expressed by that of the difference of the exposed and screened results above given. This ratio is evidently one differing a little from equality, and agreeing nearly with that of the diameters of the bulbs inversely.

(16.) The experiments now detailed will probably be considered sufficient to substantiate the conclusion; but in researches of this kind, where great numerical precision is unattainable, it seemed desirable to give the experiments that confirmation which they wanted in point of intrinsic accuracy, by frequent repetition and variation. With this view I made a great number of trials with a large differential thermometer: the bulbs were about one inch in diameter, and nearly three inches apart. The bore of the tube was about $\frac{1}{10}$th of an inch. Many of the experiments made with this instrument I shall not mention, as, although all agreeing to confirm my former conclusions, they were complicated by several unnecessary conditions.

(17.) In order to obtain results in the most simple manner, it was desirable to get rid of any action on one of the bulbs, and to expose only the other; the instrument thus acting simply as an air-thermometer. The effects on each bulb, one being painted with Indian ink, and the other coated with white silk pasted on, when exposed, might thus be compared with those through a glass screen. I first tried the experiment by placing the bulb in the focus of a spherical tin reflector about six inches diameter: by this means the source of heat could be placed at a sufficient distance to preclude any effect from the glass screen.

(18.) The experiment was again varied by placing a large opake screen before the instrument, in which was an aperture through which one bulb might be exposed. To this aperture a piece of glass could be applied. Each bulb was presented both with and without the glass.

(19.) In all these experiments it is evident, that any heating effect arising from the screen would tend to diminish the ratio of the black and white effects; and this not being allowed for in the statement of the result, the difference between this ratio

and

and that of the exposed effects will be in reality greater than appears.

The results are comprised in the following table:

(20.) Lamp. Bulb in the focus of a reflector. Coatings, white silk and Indian ink.

Experiments.	Screened.		Exposed.	
	White.	Black.	White.	Black.
1	5	8	11	15
2	4	9	13	15
3	6	11	12	14
Mean in 30 sec.	5	9·3	12	15

!1.) Incandescent iron.

1	4	7	11	13
2	4	6	10	10
Mean	4	6·5	10·5	11·5

(22.) Lamp. One bulb covered by an opake screen, the other exposed at an aperture. Distance 5 inches. Screen 1·5 inch from bulb.

1	4	6	8	11
2	3·5	6	9·5	12
3	4	6	8	10·5.
4	3	6	8	12
Mean in 1 min.	3·6	6	8·3	11·3

(23.) Incandescent iron.

In 30 sec.	3	4	12	9

(24.) It is, perhaps, not worth while to make any formal deductions from these results as to the ratio subsisting in the different cases. It will be sufficiently evident upon inspection, that, when all due allowances are made, the ratio of the effects upon the white and black bulbs is considerably greater when they were affected only by the transmissible part of the heating effect, than when they were exposed to the whole. The part, then, which is added on the removal of the screen, is of a nature tending to add to the terms of the former ratio quantities in a ratio much nearer equality: quantities in a ratio very nearly that which the effects of simple radiant heat would give.

III. (25.)

III. (25.) I have above adverted to all the sources of error which occur to me as likely to have affected these results; and when these are taken into consideration, as well as the nature of the experiments and apparatus, the accordance with the different results exhibited is perhaps as close as we can expect: and it appears that all the different sets of experiments agree in showing a very considerable difference in the *ratio* of the effects produced on a smooth black and on an absorptive white surface, by that part of the radiant effect transmitted through glass, and by the total effect. If the total direct effect were the result of one simple agent, the intervention of the glass would, by intercepting some part of it, produce no other alteration than a diminution of intensity; the *ratio* of the two effects would remain unchanged. This distinction appears to me of some importance towards clearing our ideas respecting the nature of the phænomena, and thus affording an answer to the question originally proposed in reference to some theoretical views, which, though boasting the sanction of high authority, will be untenable if the validity of these results be admitted.

(26.) The general conclusions from all these experiments may be thus recapitulated:

1st. That part of the heating effect of a luminous hot body which is capable of being transmitted in the way of direct radiation through glass, affects bodies in proportion to their *darkness of colour*, without reference to the texture of their surfaces.

2nd. That which is intercepted produces a greater effect in proportion to the *absorptive nature of texture* of the surface, without respect to colour. These two characteristics are those which distinguish simple radiant heat at all intensities.

Thus then, when a body is heated at lower temperatures, it gives off only radiant heat stopped entirely by the most transparent glass, and acting more on an absorptive white surface than on a smooth black one.

At higher temperatures the body still continues to give out radiant heat, possessing exactly the same characters. But at a certain point it begins to give out light: precisely at this point it begins also to exercise another heating power distinct from the former,—a power which is capable of passing directly through transparent screens, and which acts more on a smooth black surface than on an absorptive white one.

(27.) This last sort of heat, whatever its nature may be, is essentially different from simple radiant heat. It appears to agree very closely with what the French philosophers term *calorique lumineux*, and is, according to Professor Leslie's

theory, a conversion of light into heat. These views of the subject are certainly gratuitous assumptions. We have no right whatever to identify those two agents, or to suppose that, because a heating effect very closely accompanies the course of the rays of light, the light is therefore converted into heat: but the theories above alluded to seem to regard the *whole* heating effect of a luminous body as of this latter character. In this particular, the present inquiry has led us to an essential distinction; and if the experiments are to be relied upon, this peculiar sort of heat constitutes only *a part* of the total effect. These results do not indeed present so simple a theory as that alluded to, but they apply very obviously to the explanation of many phænomena recorded by various experimenters.

(28.) The peculiar heat above spoken of, and which for the sake of distinction and brevity we may call "transmissible heat," is similar to that which acts in the solar rays, and which there constitutes the total effect. It is this kind of heat which has been employed as a principle of photometry, on the assumption that it is precisely proportional to the intensity of light. Within certain limits this may be the case; but there are unquestionably circumstances under which the relation is very different; such, for example, as difference of colour in the light: and in general it cannot be assumed to hold good in light from different sources. To show this, there is a remarkable instance in incandescent metal, which produces but very faintly illuminating rays, yet its "transmissible heat" is very considerable. I have repeatedly tried the experiment with a small "photometer," having one bulb painted with Indian ink and the other plain: the bulbs being in a vertical line, this instrument, whether employed with or without its case or a glass screen, always gave an effect of about 10° in 30″ at eight inches distance from a ball of iron heated to the brightest point in a common fire.

(29.) In making these last experiments, the effect was always greater when the instrument was used without its case or a glass screen. This was no doubt in part owing to the greater action of the simple heat now admitted to the instrument on the coated than on the plain bulb; but it was also in part occasioned by the circumstance, that the stem going to the upper bulb passes in contact with the lower, and being a solid mass compared with the thin bulb, is slower in acquiring heat, and therefore cools it,—thus increasing the apparent effect on the other.

(30.) In a variety of other experiments which I have tried, using either this "photometer," or another having the bulbs at equal heights, various apparent anomalies presented themselves;

selves; all which I found easily explained on the principles here established of two radiations, when connected with the various other considerations to which it is necessary to refer when employing instruments of this description: but I do not conceive it necessary to enter into any further details.

LXXIII. *On the Osteology of Reptiles, and on the Geological Position of their Fossil Remains. By M.* Le Baron G. Cuvier*.

My work has necessarily resolved itself into a sort of treatise on comparative osteology, since I have been constantly obliged to bring under consideration, together with fossil bones, those of living species; nor could I have detected the differences which exist among them, otherwise than by employing figures and detailed descriptions for this purpose: but every labour devoted to the ascertainment of the differences in the productions of nature leads to the developement of their particular relations; and, indeed, the reader will have had no difficulty in perceiving that, notwithstanding the so greatly varied proportions which belong to these bones, and notwithstanding the exterior forms (often of so extraordinary a description) which hence result, there exists nevertherless, throughout the mammiferous tribes, a sort of common or universal plan, a composition nearly the same, and of a nature to enable us always to recognise each bone, by means of its use and position, through the whole of the metamorphoses under which it passes, and in spite of the difficulty presented in this recognition by the enlargements or diminutions of size which it may have undergone. Thus, in the figures of the heads which we have given, may be traced, from Man to the Whale, the frontal bones, the parietal bones, the bones of the nose,—in a word, the whole of the parts constituting of the cranium and face, with very few exceptions; such as the absence of the lachrymal bones in some species, and perhaps the inter-parietal in others. The remaining apparent differences in the number of the bones arise in general from the greater or less promptitude, or perfection of continuity, with

* From the author's *Recherches sur les Ossemens Fossiles*, vol. v. part 2. —We hope to present our readers, in continuation of this article (which contains the preliminary observations on the fossil remains of reptiles,) with a series of translations from the same work, respecting the Saurian reptiles, illustrated with engravings. M. Cuvier's Memoirs on the Osteology of living and fossil Elephants, (another interesting branch of his work,) have already been given in the Philosophical Magazine, vols. xxvi.—xxx.—Edit.

with which they unite, and effect the obliteration of the sutures which distinguish them. It is thus that the parietal, in the adult, appears sometimes simple, sometimes double, and even triple or quadruple, when we include the inter-parietals which always run into union with it, &c.* But in examining the animal at a period nearer its birth, these anomalies are found to disappear; and in the fœtus itself, or, in general, up to that period in which all the bones are as yet distinct, we find a normal number, the same in every species,—with, as I have just observed, some very rare exceptions.

It would be a curious question to ascertain whether this analogy is sustained in the other classes of the Vertebrata, and whether the differences which, in this respect, they present, depend solely upon the epochs at which their bones unite:—whether Reptiles, for instance, which always preserve in their cranium many more sutures than the mammiferous tribes, are to be considered, in this respect, as Mammalia in a state analogous to the fœtal state;—whether Birds, which in their infancy have as many sutures as Reptiles, but which, when they approach the adult state, have often fewer than the Mammifera, are, on the contrary, to be considered in this respect as Mammalia passing more rapidly from one state to the other, and advancing still further with relation to this particular circumstance the union and coalition of their bones.

M. Geoffroy Saint-Hilaire has been among the first who have entered upon this beautiful problem; and upon several points he has been most successful. I have also treated of it at various opportunities in my lectures; and I have given on those occasions in which the order of my publications have demanded it, some extracts derived from my researches upon this interesting subject†: it has, however, become the object

* N.B. It is on account of this constancy with which the inter-parietals unite at first with the parietals, before the latter join themselves to the occipital, that I now persist in assigning to them this appellation, which I had long since given to them; and against which it does not appear to me that the objections of several anatomists have been of a nature to entitle them to prevail.

† I do not pretend to contest with any one of the authors who have written upon this subject the merit of the observations which they have published: but it is my duty to protest against the affectation with which some have cited, and still continue to cite, only my *Leçons d'Anatomie*, published in 1800 by M. Duméril, after my course of lectures in 1798; and to assume the air of taking much pains to reform my opinions, after having themselves been witnesses to the innumerable preparations which have been made by me since that time, and by which more than one of them (be it said without reproach) have profited in their studies. They well knew that these preparations were already a sort of publication on my part; and it would perhaps have been just to have cited me from these latter, and not from some first, essays, which could only be considered as the sketch of a great plan.

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of the labours and more uniformly prosecuted publications of several able anatomists, particularly of MM. Oken, Spix, Bojanus, Ulrich, Rosenthal, &c.

These writers have not only endeavoured to assign to each bone in the Oviparous Vertebrata its correspondence with a bone or a determinate part of a bone in the mammiferous classes; but, conforming themselves to the ideal metaphysics and pantheism called the philosophy of nature, which has for some time enjoyed a considerable reputation in Germany,—and the language of which, as is usual in that country, the positive sciences have found themselves constrained at the moment to adopt,—they have endeavoured to discover in the head a representation of the whole body, because, in general, the principles of this philosophy require that each part, and each part of a part, should represent the whole.

It is thus that M. Oken (in his *Programme on the Signification of the Head*, Jena 1807) has quitted hold of the analogy which exists in several respects between the species of rings which form the bones of the cranium and those of the vertebræ, in order to view the cranium itself as a compound of three vertebræ *: and carrying his researches through the several portions of the head, for the purpose of ascertaining the representations offered by them of the several parts of the whole body,—he has viewed in the cranium, considered separately, the head of the head; in the nose, the thorax of the head †; in the maxillaries, the superior and inferior extremities, or the arms and legs ‡.

It will be readily comprehended that, with a little imagination, there could be no great difficulty in extracting through the medium of a principle so elevated, and moreover separated from

* The body of the anterior spheroid represents the body of the first, its orbital wings the lateral portions of the ring, and the frontal parts the spinous apophysis: this is the *ocular* vertebra:—the second or *maxillary* vertebra is similarly represented by the posterior spheroid, by its temporal wings, and by the parietals; and the third, or *auricular* vertebra, by the os basilare, the lateral occipital, and superior occipital.

† The thorax of the head is composed of the vomer, the palate, the ethmoid and nasal bones. The *cornets* of the nose are the lungs; nevertheless, the nasal cavity is a sort of prolongation of the cerebral cavity; and the nose a brain subject to the vascular system. The *smell*, which is exercised through the medium of the air, is, in the author's view, a thoracic sense; and this is the reason why no vertebra is appropriated to its use, as in the case of the *hearing*, the *taste*, and the *sight*.

‡ The two halves of the superior maxilla represent the two arms; the os quadratum (*os carré*) is the omoplate, the os pterygoides, the clavicle; the os jugale, the arm; the os maxillare, the hand; the intermaxillary, the thumb; and the teeth, the other fingers. The inferior maxilla, which in the Ovipara is composed of seven bones, will easily furnish similar indications; but

from facts by so great a distance, applications very different from the above, and even much varied among themselves.

Accordingly we find, since 1811, that M. Meckel (in his *Materials for Comparative Anatomy*, vol. ii. paper 2, p. 78) appropriates the ethmoid for the body of a vertebra, of which the frontals would form the annular part; and represents the temporals as another vertebra, of which the body would be divided into two parts (*les rochers*) by the forced introduction of the body of a third (the os basilare*).

The ethmoidal vertebra has been since adopted as a fourth, and added, under the name of *olfactive vertebra*, to the three established by M. Oken, by M. Bojanus in 1818, in the 3d number of the *Isis*, and, in 1821, in the *Parergon* of his large and beautiful work on the anatomy of the Tortoise.

M. Spix, in his great work on the composition of the head, entitled *Cephalogenesis*, and published in 1815, holds to the division of the three vertebræ of the cranium, but departs widely from the views of M. Oken relative to the bones of the face.

Representing to himself the os hyoides, the scapula, and the socket, with the extremities attached, as three circles of pieces of a similar nature, he re-discovers them in the face, attached in the same manner to the three vertebræ of the cranium. The bones which compose the nose represent to him the hyoidal and laryngial apparatus†, and those of the two maxillæ the two ordinary extremities, but with a distribution of relations quite different from that of M. Oken‡.

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but here the os carré serves as an os illium, as it had before served, in the superior maxilla, to represent the omoplate. In what follows, the real omoplate is considered as nothing else than the amalgamation of ribs, which would otherwise have been the appendices of the five last cervical vertebræ.

The os styloides is the sacrum, and forms with the os hyoides a cavity for the reception of the aliments, another being in like manner appropriated for their expulsion; and the mouth is to the abdomen what the nose is to the thorax. The lips are the instruments of touch proper to the head, as the fingers are those of the trunk.

* The zygomatic apophysis of the temporal bone would form the articular or oblique apophysis of this third vertebra; the os styloides, its transverse apophysis or rib; and the os hyoides, its sternum.

† The os planum represents, according to these definitions, the cricoides; the cribriform plate, with the crista galli, the arythenoides; the superior nasal cavities, the trachea; the inferior, the bronchiæ; the os unguis corresponds to the thyroides; and the caruncula lachrymalis, to the thymus; the bones of the palate, to the body and cornua of the os hyoides.

‡ The bones of the nose answer to the sternum; its cartilages to the xyphoides: the omoplate corresponds to what I call the posterior frontal; the clavicle to the os malæ. The shelly temporal is analogous to the os illium; the small bones of the ear represent the pubis; the cradle of the tympanum

It is probable that if other anatomists have sought for this representation of the entire body in the head alone, they may have imagined other relations, and it is by no means my intention to follow them in this branch of their researches.

The circle in which I confine myself is already vast enough to admit of our taking widely different routes, according to the point from which we may each set out. This desire of finding a representation of the body has constrained some authors to assign to a particular bone, in Reptiles or in Fishes, a denomination which they probably would not otherwise have thought of; and a predilection for discovering the osseous parts and proportions in a uniform number, has been the means of forcing others into deviations not less strange. When their calculations were foiled in the investigation of those bones in which it seemed natural to expect they might be exhibited, they found themselves necessitated to have recourse to the neighbouring ones; sometimes to admit of singular transfers, returns, and conversions, more or less complete, without regard to the innumerable organs and soft parts which it would be necessary to displace and otherwise to dispose of, in order to connect a single bone in one situation with another near it;—to insert, for example, a part belonging to the sternum between two other parts belonging to the os hyoides, or to effect such other transposition as might bear to be explained as a simple formation.

The examples of these varieties of ideas, already very numerous, relative to the Reptiles, of which I shall have to speak, might have been multiplied almost infinitely, if the limits of my work had permitted me to follow these anatomists and their sagacious emulators into the class of Fishes, and to discuss solely the several opinions which they have proposed on the constituent parts of the opercula*, and on the os hyoides.

tympanum is the ischium; the condyloidal apophysis, the os femoris; the coronoides, the tibia; &c. The teeth, according to M. Spix, are nothing else but nails,—an analogy more reasonable than that invoked by M. Oken: the alveoli are what represent the phalanges, &c. &c.

* In 1800, M. Autenrieth (in the *Zootomical Annals* of Wiedeman, vol. i. paper 2, p. 47 et seq.) considered the operculum as resulting from the division of the thyroid cartilage.

In 1807, M. Geoffroy (in the 10th volume of the *Ann. du Mus.*) supposed that the opercula were the parietals detached from the cranium.

In 1817, M. de Blainville (*Bulletin Philom.*) endeavoured to establish that the pre-operculum is the os jugale, and that the three other portions represent those which are generally found in the inferior maxilla of birds and reptiles, over and above what are found in that of fishes. M. Geoffroy opposed to this, in 1818 (in his *Philosophie Anatomique*), the jaw of a lepisosteus, which had been preserved by me, and which is quite as complicated as that of any reptile, although the lepisosteus is furnished with opercula as complete

oides*. I have at least endeavoured, in the prosecution of my labours, to avoid that description of error which is so often engendered by a preconceived theoretical opinion. I pretend not to discover a constant and unvarying number of component parts, nor the representations of parts remote from the head: I do not even pretend that the bones of the head must always be absolutely the same in each genus;—but I strive to ascertain the point to which their correspondence reaches, and the limits by which it is bounded. For this purpose I commence with the individual animal of the oviparous class, which (at least as regards the head) presents the most sensible relations to the mammifera, or to some individuals of that class:—this is the Crocodile. I describe what bones it possesses which are analogous to ours: and to establish the conclusion, I consult not only their position, but also the muscles which are attached to the part, and the nerves which pass through it, &c. I freely state what are the bones which cannot be brought within the precinct of the analogy; and I do the same thing with respect to the other genera:—I indicate where a bone, a foramen, a surface, a suture, seems to depart from the rela-

plete as those of any other fish. Nevertheless, in this same year (1818) M. Bojanus presents the same idea in the 3d number of the *Isis*, without having been acquainted with the memoir of M. de Blainville; and M. Oken bestows upon it his unqualified assent, as a thing, he observes, as certain as it is new.

In 1815, M. Spix had conceived the idea of determining the analogies of these parts of the opercula to the small bones of the ear; but in 1816 he was acutely criticized upon this subject by M. Ulrich, who regarded them as the representatives of the omoplate. This, however, did not prevent Mr. Geoffroy in 1818, in his *Philosophic Anatomique*, from arriving, on his part, at an opinion pretty closely allied to that of M. Spix, although he was unacquainted with this author's work. These two authors do not always arrange these bones in the same manner: the malleus for instance, according to M. Spix, is the pre-operculum;—according to M. Geoffroy it is the inter-operculum, &c. Lastly, M. Weber in 1820, in his dissertation *De Aure Hominis et Animalium*, has advanced another opinion quite new; which is, that the small bones which in certain fishes are attached between the cranium and air-bladder, are rigorously allied, by their functions, as well as being sometimes found similar in form, to the small bones of the ear in quadrupeds; an opinion which, sustained by new evidences in the *Isis* of 1821, is completely adopted by M. Bojanus in his *Parergon*.

* M. Autenrieth in 1800, in the same memoir in which he considers the opercula as a division of the larynx, considers the branchial rays as cartilages of the ribs, and the osseous branches which carry them he considers as formed by the os hyoides and some parts of the sternum. M. Geoffroy, on his part, in 1807, and without any knowledge of the labours of M. Autenrieth, conceives ideas possessing a very close resemblance to the above, which he has developed more in detail in his *Philosophie Anatomique*, and which constitute the foundation, and afford the starting-point, of his theory of the branchial apparatus.

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tion in question; and I mark where it appears to present anew the same relation. Having no occasion to display things other than they are, I employ neither those vague propositions nor those figurative expressions by which I might deceive myself, as has happened to so many other individuals of the strictest integrity of mind: and if, by these means, I arrive not at such brilliant results, I flatter myself that I tread a more solid path.

With respect to the head, it is, as I have above observed, upon the Crocodile that I have found it necessary to insist the most; for, the bones of this animal once named, we find it easy to name those of the Tortoises, the Lizards, and the greater part of the Serpents: but a new and more difficult study becomes necessary in considering the Batracian tribes.

The bones of the shoulder and the sternum must be studied, especially in the Lizards, in which they present the greatest degree of complication.

In regard to the os hyoides, it is among the Batracians that it possesses the most importance; since it there furnishes us with the means of forming clear ideas upon that of Fishes; on which subject numerous and very diversified systems have been conceived.

Upon this point, I trust that the facts adduced in this part of my work, and especially the successive simplification and ultimate disappearance of the auricular apparatus, as well as the gradual development of the hyoidal apparatus, in the Batracians, notwithstanding the presence of a larynx and a sternum, will lead us back to the former views,—those which I have constantly announced,—that the bones of the ear do not re-appear in osseous Fishes in the form of opercula; that the branchial apparatus, in order to present the complication which characterizes it, has no need of completion by means of intercalary sternal, laryngial, or costal pieces; and, finally, that the opercular apparatus is one which is in itself special, and proper to the species in which it is discovered.

I will here add but one word on the other parts of the body; which is, that the particular sections which constitute each part, so far from being multiplied like those of the head, have not, in the infant state, those productions at their extremities which we call *epiphyses*.

In the Crocodiles and Tortoises, the extremities of the bones and their principal eminences are overlaid or surrounded with cartilages, which harden and ossify with age; but in which there is not formed, as in the Mammifera, an osseous kernel, separated for a time from the bone itself, or from the diaphysis, by a suture; a circumstance the more remarkable, from the

the Lizards, especially the Monitors, having well-marked epiphyses appended to their long bones.

It is in each genus, after having thus studied and brought as far as practicable under general rules the osteology of living reptiles, that I pass to the examination of the fossil bones most analogous; and in this part of my labour I am equally led into considerations of a much more extended nature than had been suggested by investigating the osseous structures of the Mammifera.

The Mammalia, as they are the last, so they are the most perfect products of creative power.

The existence of Reptiles commenced much earlier: the vestiges of these latter are imbedded in formations of a more ancient date, and the naturalist is obliged to follow their remains through strata of greater depth.

It has been seen, in my preceding volumes, that beyond comparison the greatest number of viviparous quadrupeds have left vestiges of their bones in the last alluvial beds or in caverns, or lastly in breaks or openings of rocks, that the sea, which has flowed over them, has scarcely had time sufficient allowed it to deposit the traces of its influx; or at least, that it has not covered them anew with any solid and regular beds. Some local formations only, and which appear to be of a more ancient date, include principally unknown genera, and are in some places overlaid by marine strata. But in our *calcaire grossier*, our *calcaire à cérithes*, we hitherto discover no other Mammifera than those of the sea,—Phocæ, Lamantines, and Cetacea. One sole exception, probably the result of mistake, invades this rule: these are the plastic clay beds, the lignites which they contain, and some other lignites contemporary with these, in which bones, incontestably those of Mammalia, are observed; in which I have indeed found my Antracotheriums, and some Palæotheriums, accompanied, as in our gypsums, with the Trionyx and Crocodile; in which I have recently recognised the bones and teeth of the Mastodon, and the jaw of the Beaver*. These beds, these lignites would be, it is urged, uniformly inferior to our *calcaire grossier;* but this inferiority, were it as well ascertained as it appears doubtful, and were it true that lignites and their containing strata of two distinct epochs have not been confounded together, we

* I owe the communication of the fragments of the Mastodon to the count Vitalien Borromeo, of Milan, and that of the jaw of the Beaver to my scientific friend M. Brongniart. All these remains are derived from the lignites of Horgen. It is Professor Meissner, of Bern, who appears to have been the first to discover in them the existence of fossil bones.

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should still be obliged to maintain that strata (which it is on all hands agreed are incumbent upon chalk) are most ancient where they present the remains of Mammifera,—that the chalk is hitherto found to contain absolutely none,—and that in the older strata these remains still less exist; while, nevertheless, chalk and also the greatest part of those older strata, even to the great coal formation, abound, in certain localities, with Tortoises, Lizards, and Crocodiles, species which are, on the contrary, exceedingly rare in the superficial beds.

We ascend then, to another age of the world,—to that age in which the earth was as yet traversed only by the cold-blooded reptiles,—in which the sea abounded in ammonites, belemnites, terebratulæ, and encrinites,—and in which all these genera (at this day subjects of prodigious rarity) formed the mass of its population.

This is that age which geologists have named the epoch of the secondary strata. It would perhaps be appropriate to the nature of our work, here to enter upon an enumeration of these strata, and a description of their nature and superposition, similar to that which we have already given of the tertiary strata in our second volume, with relation to the bones in our plaster quarries at Paris: but this task has been so fully accomplished, (and by geologists better located than we are for effecting it,) that we cannot do better than to refer our readers to the excellent works which have recently appeared upon this subject. It is not, in fact, in the canton which we inhabit, but on the other side of the vast circumference of chalk which surrounds us, that the secondary strata present themselves in sufficient relief to be commodiously studied: it is between the chalk and the primitive strata that they ascend into day; and Germany on the one side, and England on the other, are the two theatres in which it has been possible to verify their succession, and to form a history of them in some degree complete.

Werner commenced by this study the great reform which he has introduced in geology; and the more extended researches of his pupils, and principally of MM. de Buch and de Humboldt, have brought this work to the greatest state of perfection. The results have been faithfully presented, in our language, in the work of M. Bonnard, entitled *Aperçu Géognostique des Terrains;* and M. de Humboldt has recently presented them anew, with still further details, and an extended series of observations, as remarkable for their excellence as for their novelty, in his *Essai Géognostique sur le Gisement des Roches.* A series of analogous observations has been followed up with great perseverance in England by the members of the Geological Society of London; and the disposition of these

these formations, such as it exists in that country, was presented, in 1816, in the tables of Mr. Buckland, and in 1822 in the excellent work of Messrs. Conybeare and Phillips, entitled *Outlines of the Geology of England and Wales.*

It therefore only remains to establish upon a more certain basis the concordance and harmony of the different systems of formations observed by one and by the other; and it is to this point that the united efforts of observers constantly tend, and to which they will ere long conduct us.

In the mean time I may refer, during the sequel of the present researches, to the two principal works above cited,—that of M. de Humboldt, and that of Messrs. Conybeare and Phillips; and it is to these I shall direct the attention of my readers for the proofs of the respective positions of the fossils of which I am about to treat.

As in my history of the bones of the Mammifera, so in the present, the order which I shall observe will be neither entirely geological nor entirely zoological.

I commence with the Crocodiles, because it is their osteology which serves as the point of departure, and because their bones are found in the greatest number of formations, and are recognized therein with the greatest degree of facility.

The Tortoises next follow, whose size has caused them to be noticed in numerous localities, and who, by the osteology of their head, as well as by many details of organization, approximate at least equally with the Crocodiles to the class of Mammifera.

The Lizards will be included in the third chapter; and will offer to us extraordinary conformations, worthy of our utmost attention.

It will be in my power to give but little space to the bones of *Serpents* and those of *Birds*, which are but rarely met with among fossils; but I shall treat in detail the *Batracians*, not merely on account of the remarkable species of this family which has long been taken for a fossil human skeleton, but also because it is in their anatomy that the greatest number of errors have been committed, and the greatest number of suppositions and systems void of foundation entertained. Nevertheless, this anatomy is among the most important, since it is what conducts us to the explication of that of Fishes.

It is after having thus studied the osteology of the still existing families of Reptiles that I pass to the examination of a lost family—one more extraordinary perhaps than any of the whole of those alluded to in my work;—of those *Ichthyosauri* recently discovered in England, and in which are united characters so singularly combined, that from the aspect of some of

of their parts we are tempted to confound them with the Cetacea or the Fishes; and it is only by a knowledge extended into the entire of their skeleton that we are enabled to convince ourselves of the necessity of classing them with the other reptiles.

It is with those I shall terminate the eighth and concluding part of my book, reserving for consideration in a future work, should my health and occupation permit, the subject of the fossil bones of Fishes.

LXXIV. *Notices respecting New Books.*

Recently published.

THE Third Part of the Philosophical Transactions for 1824 has just appeared; with Part I. for the present year. The former contains,

Observations of the apparent distances and positions of 380 double and triple stars, made in the years 1821, 1822, and 1823, and compared with those of other astronomers; together with an account of such changes as appear to have taken place in them since their first discovery. Also a description of a five-feet equatorial instrument employed in the observations. By John Frederick William Herschel, Esq. F.R.S., and James South, Esq. F.R.S. This paper occupies above 400 pages.

Part I. for 1825 contains the following papers:

On the effects of temperature on the intensity of magnetic forces; and on the diurnal variation of the terrestrial magnetic intensity. By Samuel Hunter Christie, Esq. M.A. of Trinity College, Cambridge, Fellow of the Cambridge Philosophical Society: of the Royal Military Academy. Communicated by the President.—The Croonian Lecture. On the existence of nerves in the placenta. By Sir Everard Home, Bart. V.P.R.S. —Observations on the changes the ovum of the frog undergoes during the formation of the tadpole. By Sir Everard Home, Bart. V.P.R.S.—A general method of calculating the angles made by any planes of crystals, and the laws according to which they are formed. By the Rev. W. Whewell, F.R.S. Fellow of Trinity College, Cambridge.—Explanation of an optical deception in the appearance of the spokes of a wheel seen through vertical apertures. By P. M. Roget, M.D. F.R.S.—On a new photometer, with its application to determine the relative intensities of artificial light, &c. By Wm. Ritchie, A.M. Rector of the Academy at Tain. Communicated by the President.—The description of a floating collimator. By Captain Henry Kater, F.R.S.—Notice on the Iguanodon, a newly discovered fossil reptile, from the sand-

 stone

stone of Tilgate Forest, in Sussex. By Gideon Mantell, F.L.S. M.G.S. &c.—An experimental inquiry into the nature of the radiant heating effects from terrestrial sources. By Baden Powell, M.A. F.R.S. of Oriel College, Oxford.

No. I. of *Annulosa Javanica;* or, An Attempt to illustrate the Natural Affinities and Analogies of the Insects collected in Java by Thomas Horsfield, M.D. F.L. & G.S., and deposited by him in the Museum of the Honourable East India Company. By W. S. MacLeay, Esq. M.A. F.L.S. &c. &c.

This work, which is intended to contain systematic descriptions of all the Insects collected by Dr. Horsfield in Java, is to be published in numbers. The species are to be arranged, as nearly as possible, according to their natural affinities; and in order to make this, the important part of the science, more clear, the descriptions are to be interspersed with such leading observations on the economy and anatomical structure of the families, as may render the work interesting to naturalists in general, as well as to the entomologist.

The second number is now in progress for publication, and will contain the whole of the Coleoptera having Iuliform larvæ.

The Insects described are arranged in the Museum of the Honourable East India Company, where it is stated they may be inspected under the regulations established at their library.

We shall have to recur to this valuable and interesting work in a future number, as it contains much that is of great importance to those who take an interest in general views of Natural Arrangement.

Part I. of Dr. Alex. Jamieson's New Practical Dictionary of Mechanical Science, embellished with many hundred engravings on copper and wood.

Preparing for Publication.

Mr. G. Poulett Scrope has in the press A Treatise on Volcanoes, and on their Connexion with the History of the Globe: in one octavo volume, to be illustrated by plates and numerous engravings on wood.

LXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THE following papers have been read during the present month:

June 2.—Microscopical observations on the materials of the brain, ova, and testicular secretions of animals, to show the analogy that exists between them. By Sir Everard Home, Bart. V.P.R.S.

June 9.

June 9.—Description of a method of determining the direction of the meridian. By John Pond, Esq. F.R.S. Ast. Roy. —Further researches on the preservation of metals by electrochemical means. By Sir Humphry Davy, Bart. P.R.S.

At this meeting, MM. Bessel, Encke, Chaptal, Fresnel, and Brongniart, were elected *foreign members* of the Society.

June 16.—On some new compounds of carbon and hydrogen, and on certain other products obtained during the decomposition of oil by heat. By M. Faraday, F.R.S.—Account of the repetition of Mr. Arago's experiments on the magnetism developed in various substances during the act of rotation. By Charles Babbage, Esq. F.R.S. and J. F. W. Herschel, Esq. Sec. R.S.—Experiments on magnetism produced by rotation. By S. H. Christie, Esq. M.A. F.R.S.—On the annual variation of some of the principal fixed stars, By John Pond, Esq.—Description of an improved hygrometer. By Mr. Thomas Jones. Communicated by Capt. Kater, F.R.S.—On the nature of the function of mortality, and on a new mode of determining the value of life contingencies. By Benjamin Gompertz, Esq. F.R.S.

The Society then adjourned to the 17th of November next.

LINNÆAN SOCIETY.

June 7.—Some communications were read from Lieut. J. H. Davies, and Charles Wilcox, Esq. relative to a species of *Mitylus* (*M. bidens*) found in great quantity adhering to the bottom of H. M. ship Wellesley, built at Bombay, and which has been lying in Portsmouth harbour ever since 1816. It seems to be quite naturalized there, and to propagate abundantly. Also a paper On the crepitacula and organs of sound in orthopterous insects; and particularly in the *Locusta camellifolia*, a description of which is subjoined. By the Rev. Lansdown Guilding, B.A. F.L.S.

June 21.—Read, A descriptive catalogue of the Australian birds in the cabinet of the Linnæan Society. By Thos. Horsfield, M.D. F.L.S., and N. A. Vigors, Esq. F.L.S. In the introductory remarks, the writers express their confident expectation that the deficiency of our knowledge of the habits of the birds of Australia may be in great measure supplied by the exertions of Mr. MacLeay during his residence in that country, for which he is shortly about to depart.—Read also, A notice on a peculiar property of a species of Echinus. By E. T. Bennett, F.L.S. Communicated by the Zoological Club of the Linnæan Society.

GEOLOGICAL SOCIETY.

May 6.—A paper was read entitled "A brief description of an extensive hollow or fissure, recently discovered at the quarries near the extremity of the Western Hoe, Plymouth;" by the Rev. Richard Hennah.

In this communication the author describes an extensive hollow or cave in the limestone rocks near Plymouth, in which no remarkable bones have yet been discovered, but in which stalactites are particularly abundant. Mr. Hennah offers some remarks on the various causes and circumstances which have contributed to give to these stalactites their different shapes and compositions.

A paper entitled "On a dyke of serpentine, cutting through sandstone in the county of Forfar;" by Charles Lyell, Esq. Sec. G.S. was read in part.

May 20.—The above paper was concluded.

In the former part of this paper, the rocks which are exposed on the left bank of the Carity, a small river in Forfarshire which descends from the mica-schist district of the Grampians into Strathmore, are described. The first of these is a clay-stone porphyry; next to it is a conglomerate containing quartz pebbles; and then strata of fine-grained micaceous sandstone and shale dipping to the south, and which are suddenly cut off at an angle by the serpentine. These strata of sandstone and shale form part of a great series which overlies the clay-slate, to which it immediately succeeds; and is older than the great conglomerate of the old red sandstone, which lies immediately upon it. The serpentine is vertical, and is well characterized. It contains in part veins of asbestos, and in parts diallage, and a large mass of hypersthene. On the other side of this dyke of serpentine, which is 90 yards thick, fine-grained sandstone and conglomerate again appear, and slip away from the serpentine towards the south. Next to these, a mass of serpentine is seen mixed with dolomite, and at its side altered sandstone and a conglomerate in which the quartz pebbles are split and re-united by ferruginous matter. Lastly, at a short distance a dyke of greenstone parallel to the serpentine occurs, flanked on both sides by vertical masses of sandstone and conglomerate much altered and indurated, and charged with brown spar.

Mr. Lyell next describes the rocks on the right bank of the river, which resemble those on the left, with one exception, viz. that the great dyke of serpentine seems to be connected with the mass of dolomitic serpentine, a thin bed of fine-grained greenstone alone intervening, and the sandstone and conglomerate,

merate, which appeared between them on the opposite side, being absent.

In conclusion, the author traces this dyke of serpentine, pursuing its course in a direct line to the N.E. and S.W. of the locality in which it occurs on the Carity. It is found recurring at intervals for the space of at least fourteen miles from the bridge of Cortachie to Bamff, near Alyth, in Perthshire. It is always unconformable to the strata through which it passes, and its course is never interrupted by any other rock.

A notice was then read, On the serpentine of Predazzo; by J. F. W. Herschel, Esq.

In this communication the author mentions that at Canzocoli, near Predazzo, in the Tyrol, where a junction is seen of a granite-form sienite with dolomite, a layer of serpentine is found to intervene between the sienite and the dolomite.

The dolomite dips at an angle of 50° or 60° beneath the sienite, and near the junction an alternation takes place in its mineralogical character; as it presents, instead of its usual highly crystallized saccharine structure, a flaky and very talcose appearance. The incumbent sienite is no less affected. Its grain is smaller, and it is intersected with innumerable veins parallel to the plane of junction, of a white mealy substance, which partly dissolves with effervescence, and partly gelatinizes with nitric acid. In the midst of this white substance occurs the thin lamina of serpentine, which is extremely well characterized. The whole of the transition from the sienite to the dolomite takes place within a thickness of about 18 inches or 2 feet.

A notice was read, On carbonate of copper, occurring in the magnesian limestone at Newton Kyme, near Tadcaster; by William Marshall, Esq. M.G.S.

The green carbonate of copper, found by the author in a large quarry of magnesian limestone near Tadcaster, runs through the limestone in thin veins, dipping to the west, the dip of the limestone being in the same direction, but at a less angle. At Farnham, a small village two miles N.W. of Knaresborough, which is also in the magnesian limestone, a considerable quantity of copper was formerly obtained: and these are the only two instances in which Mr. Marshall has heard of any of the ores of copper having been found in the magnesian limestone.

June 3.—A paper was read, entitled "Remarks on quadrupeds imbedded in recent alluvial strata;" by C. Lyell, Esq., Sec. G.S.

In a former communication to the Society, the author had stated that he had found it difficult to explain the circumstances under

under which the remains of quadrupeds were very generally found imbedded in the shell marle in Scotland, often at considerable depths, and far from the borders of those lakes in which the marle is accumulated.

These animals must have been drowned when the lakes were of a certain depth. Their bones are found in the marle, unaccompanied by sand or gravel or any proofs of disturbing forces. From the shape of the surrounding land in some instances, it appears that floods could not have swept them in; and from the occasional absence of rivers flowing into others, they could not have been washed in by them.

The author therefore suggests that they were lost in attempting to cross the ice in winter, the water never freezing sufficiently hard above the springs to bear their weight, and springs abounding always in those lakes in Forfarshire and Perthshire in which marle is deposited.

The skeletons of some of the animals found in the shell marle in Forfarshire are in a vertical position, but some are not. The same circumstance has been remarked with regard to the elks occurring in the marle in the Isle of Man. Of these facts Mr. Lyell offers the following explanation.

Cattle which are lost in bogs and marshes sink in and die in an erect posture, and are often found with their heads only appearing above the surface of the ground. When therefore a lake in which marle is deposited is shallow, the quadrupeds which fall through the ice sink into the marle in the same manner, and perish in an upright posture, but when the lake is deep and the animals are dead before they reach the bottom, they become enveloped in the marle in any position rather than the vertical.

HORTICULTURAL SOCIETY.

May 2.—The Anniversary of the Society, the President (Thomas Andrew Knight, Esq.) in the Chair. The following were chosen officers for the ensuing year.

President: Thomas Andrew Knight, Esq.—*Treasurer:* John Elliot, Esq.—*Secretary:* Joseph Sabine, Esq.—*Assistant Secretary:* Mr. John Turner.

The following gentlemen were elected into the vacancies in the Council.

The Duke of Bedford; Alexander Seton, Esq.; Mr. James Young.

The following Members of the Council were appointed Vice-Presidents for the ensuing year.

The Earl of Aberdeen; John Elliot, Esq.; Robert Henry Jenkinson, Esq.; Alexander Henderson, M.D.

May 3.

May 3.—A paper was read on the construction of pine pits worked by steam. By Mr. William M'Murtrie.

May 17.—The following papers were read: Description of a grape-house adapted for early forcing, by Mr. A. Wilson. —Description of American fruits, of which trees have been transmitted to the garden of the Horticultural Society of London, by Mr. Michael Floy of New York.—On the cultivation of strawberries, by the President.

ASTRONOMICAL SOCIETY OF LONDON.

June 10.—The reading of Mr. F. Baily's Introduction to his new Tables for determining the apparent places of about 3000 fixed stars was resumed and completed. This copious introduction commences with a historic sketch of the most important tables which have hitherto been published for similar purposes; none of which, however, are so extensive as the tables to which the present paper is introductory. They comprehend, first, all stars, not less than the *fifth* magnitude, wherever situated; secondly, all the stars, not less than the *sixth* magnitude, situated within 30° of the *equator;* thirdly, all the stars, not less than the *seventh* magnitude, situated within 10° of the *ecliptic.*

After a few general observations, Mr. Baily speaks, in succession, of the distinct topics of *aberration*, *annual precession*, and *nutation;* exhibiting the analytical formulæ which have been proposed for the computation of their respective values at any time (past, present, or future); assigning the reasons for the adoption of those values of the *constants* which he has preferred; and so transforming the several formulæ, as to facilitate and effect their reduction into one class, of comparative simplicity, which forms the basis of the tables themselves. Thus the total corrections for right ascension and declination respectively, assume the forms

$$\Delta\,\alpha = \mathrm{A}\,a + \mathrm{B}\,b + \mathrm{C}\,c + \mathrm{D}\,d$$
$$\Delta\,\delta = \mathrm{A}\,a' + \mathrm{B}\,b' + \mathrm{C}\,c' + \mathrm{D}\,d'$$

where the quantities denoted by a, b, c, d, and the accentuated a', b', c', d', are *constant* for each star, while the quantities A, B, C, D, are common to every star. The quantities A, B, are rendered equally *constant* for all the stars by the assumption of a fictitious year, commencing at that moment when the sun's *mean* longitude at Greenwich at *mean* noon on Jan. 1 is 281°; which is, therefore, assumed as the *tabular date;* and the mode of adopting it to the current date is explained.

The author then explains the arrangement and use of the tables. The general catalogue of the stars is arranged in the order of the right ascensions, and reduced to Jan. 1, 1830. The left

left hand page is confined to the *right ascensions*, the right hand page to the *declinations*. Col. 1, on the left hand, exhibits the numbers of the stars. Col. 2, the *names*; to which are prefixed Flamsteed's numbers, and the letters of the alphabet, by which they are usually distinguished. Col. 3 denotes the *magnitudes* of the stars. Col. 4, the right ascensions, *in time*, for Jan. 1, 1830. Col. 5, the annual precession *in time*. The remaining columns contain the logs. of a, b, c, d, each previously divided by 15 to reduce them to time.

On the right hand page, Col. 1 is the same as Col. 1 on the left hand. Col. 2 exhibits the *declinations* of the stars for Jan. 1, 1830. Col. 3 the *annual precession*. Cols. 4, 5, 6, 7, the values of a', b', c', d'. Then there are two columns headed B and P, denoting the corresponding numbers in the catalogues of Bessel and Piazzi respectively; while the last column is reserved for those which are to be found in Hevelius, Lacaille, Mayer, Zach, &c.

There are several subsidiary tables, which Mr. Baily also succinctly explains; and he further developes the principles of correction for *proper motion*, &c. when necessary.

The general rule for the use of the tables is this: viz. Take out from the general catalogue, and opposite the given star, the logarithms of a, b, c, d, and a', b', c', d', with their proper signs; and from the subsidiary Tables I. and II. opposite the given day, the logs. of A, B, C, D, with their proper signs; which must be written down under the preceding logarithms: then add each pair A, a; B, b, &c.; together; and take out respectively the natural numbers corresponding to the sum of the two logarithms; and (observing that the signs only affect the resulting natural numbers) incorporate them by addition or subtraction accordingly; the amount will be the total correction required; that arising from a, b, c, d, being the correction in Æ; that from a', b', c', d', the correction in declination. Thus, the whole of the corrections are obtained without a reference to any other work, except a *small* table of logarithms.

The tables are arranged to *mean solar* time, which, it is presumed, will extend their utility. And it may be observed, that, by way of *artificial memory*, to facilitate the recollection of the precise subject to which each column refers (as in B for Bessel, P for Piazzi, already mentioned), Mr. Baily has made A B represent the quantity by which the AB*erration* is determined, C the quantity by which the *pre*C*ession* is determined, and D that by which the D*eviation* (or *Nutation*) is determined. These contrivances, though avowedly subordinate, will not be despised by those who know how much the

pursuits

pursuits of science are at all times promoted by the introduction of a happy technical mnemonics.

After the reading of this elaborate and interesting paper, the Society adjourned to Friday the 11th of November next.

ROYAL ACADEMY OF SCIENCES OF PARIS.

March 14.—M. Déyeux made a report on the means for preserving fresh butter: (these means are neither novel nor efficacious.)—M. Mathieu gave an account of a work on Perspective, and of its application to surveying and to military investigations, which M. Boscary, an officer in the artillery service, had presented in December last. The author will be invited to continue his researches.—M. de Humboldt concluded the reading of his memoir on some physical phænomena presented by the Cordillera of the Andes of Quito and the eastern part of the Himalayah.—M. Cuvier read a memoir on the Myripristis, a new genus of fish of the family of the Perches, remarkable by the connexion of its swimming-bladder and of its ear.—M. Auzout presented a specimen of artificial anatomy.—M. Babinet read a memoir on a new method of discovering the masses of the planets.

March 21.—The Academy received the following works in manuscript: Memoir on the composition of new water-cements, and on the general theory of mortars, by M. Girard; Treatise on the hyper-archisophic and the olo-palingenic calculus, by an anonymous writer; New arrangement of the animal kingdom, by M. Lamouroux.—M. de Humboldt presented a new vertical section of the south of Germany and of France, extending from the Black Forest to Paris, founded on a barometric levelling by MM. d'Ogelausen, La Roche, and Decheux, engineers *du Corps des Mines* of Prussia.—M. Cauchy made a report on a memoir by M. Brisson, entitled, "New researches relative to the integral calculus of partial differences:" The conclusions of this report were not adopted by the Academy.—M. Cuvier read a memoir on the fresh-water fishes of India, which have the faculty of living a long time out of water; and on the organs from which they derive this faculty.—M. Guillemain read a memoir on the pollen of plants and on the generation of vegetables.

March 28.—M. de Humboldt presented to the Academy, in the names of MM. Nœggerath and Bischof, a specimen of meteoric iron, weighing 3400 pounds, found on the summit of a hill at Bitburg, near Trèves. This mass contains nickel and sulphur, but neither chrome nor carbon*.—M. G. de St.

* MM. Nœggerath's and Bischof's memoir on this mass forms the first article of our present Number.—Edit.

Hilaire exhibited the head of a monstrous colt, foaled two days before at the Royal School of Alfort.—M. Gaudin, of Nantes, presented a memoir on the nature of negative quantities.—M. Traullé read a sketch on the Deluge, on its consequences, and its producing cause; and on the occurrence in the north of the two continents of the bones of animals belonging to southern climates.

LXXVI. *Intelligence and Miscellaneous Articles.*

ASTRONOMICAL INFORMATION.

PROFESSOR SCHUMACHER, whose whole time seems devoted to the promotion of astronomy, has recently published a work which will be highly useful to astronomers in general. It may be recollected by most of our readers, that the French Government published in 1801 a work containing *fifty thousand* observations of stars, made at the *Ecole Militaire* in Paris, by Lefrançois Delalande, nephew of the celebrated Lalande, between the years 1791 and 1801, with a quadrant having a radius of 7½ feet. About 12,000 of these stars have been reduced, and published at various times in the volumes of the *Connaissance de Tems;* but the remainder have been of little or no use to astronomy, on account of the labour of determining the corrections necessary to be applied to bring them up to a given epoch. This labour is saved by means of the work in question. It is entitled *Tafeln zur Reduction der in der Histoire Céleste enthaltenen beobachtungen*, in one small volume octavo. The plan was first suggested by M. Bessel in the *Astron. Nachrichten*, No. 2; and has been executed by Messrs. Hansen and Nissen, under the direction of M. Schumacher. The rule for the reduction is very simple; being nothing more than the addition of three quantities found in the tables, and which are taken almost by inspection. So that a person may, in a very few seconds, reduce any of the observations to the common epoch 1800. The work is dedicated to the *Astronomical Society of London.*

NEW COMET.

M. Gambart, director of the observatory at Marseilles, discovered a small comet in the head of *Cassiopea* on the morning of May 19th. It appeared as a *nebula* of about 2′ in diameter; round, and well defined. Its right ascension at that time was $0^h\ 20^m$, and its declination 48° 22′ north. On June 1st, about midnight, its right ascension was $1^h\ 51^m$, and its declination

declination 73° 29. So that its motion in declination appears to be about 2° per day. We do not find that it has yet been seen in this country.

HEIGHTS OF PLACES IN PREANG (JAVA) REGENCY. MEASURED BY M. REINWARDT.

	English feet.
Buitenzorg	865
Megamendon	4,848
Salak	7,172
Gede	9,075
Pontjak Karang (dist. of Tjihea)	2,774
Patocha (dist. of Tjisondarie)	7,407
Tombak Ræijong (ditto)	6,291
Village of Tjiwednij (ditto)	3,572
Northern Peak of Tiloe (dist. of Banjaran)	5,425
Southern Peak of ditto (ditto)	6,034
Kampong Lamadjam (ditto)	3,169
Kampong Malabar (ditto)	3,363
Mountain of ditto (ditto)	6,621
Village of Banjaran (ditto)	2,534
Kampong Marajon (dist. of Tjiparay)	3,035
Kanpong Nenkellen (ditto)	3,742
Head of the Tjitarum river (dist. of Manahaija)	4,645
Sumbong (ditto)	5,593
Tjikaraha (ditto)	4,017
Goenong Goentoer (dist. of Timanganten)	6,085
Telaga Bodas (dist. of Wanaradja)	5,497
Village of Trogong (dist. of Timanganten)	2,350

Verhandel. v. het Batav. Genootschap.—Asiat. Journ.

HYÆNA CAVES IN DEVONSHIRE.

Professor Buckland has lately examined two caves in Devonshire, in both of which he found, in a bed of mud beneath a crust of calc-sinster, gnawed fragments and splinters of bones, with teeth of hyænas and bears. There were no entire bones, except the solid ones of the toes, heels, &c., as at Kirkdale, which were too hard for the teeth of the hyæna. They appear simply to have been dens, but less abundantly inhabited than that of Kirkdale. In the same cave, Professor Buckland found one tooth of the rhinoceros and two or three only of the horse.—*Edin. Phil. Journ.*

CURIOUS ANECDOTE OF A HEDGEHOG.

The following curious circumstance, witnessed by Professor Buckland

Buckland, is related in a paper on the Habits of Animals, by Mr. Broderip, in the *Zoological Journal*, No. V.

Having occasion to suspect that hedgehogs, occasionally at least, preyed on snakes, the Professor procured a common snake (*Coluber natrix*) and also a hedgehog which had lived in an undomesticated state some time in the Botanic garden at Oxford, where it was not likely to have seen snakes, and put the animals into a box together. The hedgehog was rolled up at their first meeting: but the snake was in continual motion, creeping round the box as if in order to make its escape. Whether or not it recognised its enemy was not apparent; it did not dart from the hedgehog, but kept creeping gently round the box; the hedgehog remained rolled up, and did not appear to see the snake. The Professor then laid the hedgehog on the body of the snake, with that part of the ball where the head and tail meet downwards, and touching it. The snake proceeded to crawl,—the hedgehog started, opened slightly—and, seeing what was under it, gave the snake a hard bite, and instantly rolled itself up again. It soon opened a second time, repeated the bite, then closed as if for defence; opened carefully a third time, and then inflicted a third bite, by which the back of the snake was broken. This done, the hedgehog stood by the snake's side, and passed the whole body of the snake successively through its jaws, cracking it, and breaking the bones at intervals of half an inch or more; by which operation the snake was rendered entirely motionless. The hedgehog then placed itself at the tip of the snake's tail, and began to eat upwards, as one would eat a radish, without intermission, but slowly, till half of the snake was devoured, when the hedgehog ceased from mere repletion. During the following night the anterior half of the snake was also completely eaten up.

EXPLANATION OF AN OPTICAL DECEPTION: BY DR. ROGET.

A curious optical deception takes place when a carriage-wheel, rolling along the ground, is viewed through the intervals of a series of vertical bars, such as those of a palisade, or of a Venetian window-blind. Under these circumstances the spokes of the wheel, instead of appearing straight, as they would naturally do if no bars intervened, seem to have a considerable degree of curvature. The distinctness of this appearance is influenced by several circumstances presently to be noticed; but when every thing concurs to favour it, the illusion is irresistible, and, from the difficulty of detecting its real cause, is exceedingly striking.

The degree of curvature in each spoke varies according to the situation it occupies for the moment with respect to the perpendicular

perpendicular. The two spokes which arrive at the vertical position, above and below the axle, are seen of their natural shape, that is, without any curvature. Those on each side of the upper one appear slightly curved; those more remote, still more so: and the curvature of the spokes increases as we follow them downwards on each side till we arrive at the lowest spoke, which, like the first, again appears straight.

The most remarkable circumstance relating to this visual deception is, that the convexity of these curved images of the spokes is always turned downwards on both sides of the wheel; and that this direction of their curvature is precisely the same, whether the wheel be moving to the right or to the left of the spectator.

In order to discover a clue to the explanation of this phænomenon, it was necessary to observe the influence which certain variations of circumstances might have upon it; and the following are the principal results of the experiments I made for this purpose.

1. A certain degree of velocity in the wheel is necessary to produce the deception above described. If this velocity be gradually communicated, the appearance of curvature is first perceptible in the spokes which have a horizontal position: and as soon as this is observed, a small increase given to the velocity of the wheel produces *suddenly* the appearance of curvature in all the lateral spokes. The degree of curvature remains precisely the same as at first, whatever greater velocity be given to the wheel, provided it be not so great as to prevent the eye from following the spokes distinctly as they revolve; for it is evident that the rapidity of revolution may be such as to render the spokes invisible. It is also to be noticed that, however rapidly the wheel revolves, each individual spoke, appears, during the moment it is viewed, to be at rest.

2. The number of spokes in the wheel makes no difference in the degree of curvature they exhibit.

3. The appearance of curvature is more perfectly seen when the interval between the bars through which the wheel is viewed are narrow, provided they are sufficiently wide to allow of the distinct view of all the parts of the wheel in succession, as it passes along. For the same reason, the phænomenon is seen to the greatest advantage when the bars are of a dark colour, or shaded, and when a strong light is thrown upon the wheel. The deception is in like manner aided by every circumstance which tends to abstract the attention from the bars, and to fix it upon the wheel.

4. If the number of bars be increased in the same given space, no other difference will result than a greater multiplication

tion of the curved images of the spokes; but if a certain relation be preserved between the angles subtended at the eye by the whole intervals of the bars, and of the extremities of the spokes, this multiplication of images may be corrected. The distance of the wheel from the bars is of no consequence, unless the latter are very near the eye, as in that case the apertures between them may allow too large a portion of the wheel to be seen at once.

5. If the bars, instead of being vertical, are inclined to the horizon, the same general appearances result, but with this difference, that the spokes occupying positions parallel to the bars are those which have no apparent curvature; while the curvatures of the other spokes bear the same relations to those straight spokes, and to each other, that they did in the former case. When the inclination of the bars is considerable, however, the images become more crowded, and the distinctness of the appearance is thereby diminished. The deception totally ceases when the wheel is viewed through bars that are parallel to the line of its motion.

6. It is essential to the production of this effect that a combination should take place of a progressive with a rotatory motion. Thus, it will not take place if, when the bars are stationary, the wheel simply revolves on its axis, without at the same time advancing; nor, when it simply moves horizontally, without revolving. On the other hand, if a progressive motion be given to the bars while the wheel revolves round a fixed axis, the spokes immediately assume a curved appearance. The same effect will also result if the revolving wheel be viewed through fixed bars by a spectator who is himself moving either to the right or left, because such a movement on the part of the spectator produces in his field of vision an alteration in the relative situation of the bars and wheel.

It is evident from the facts above stated, that the deception in the appearance of the spokes must arise from the circumstance of separate parts only of each spoke being seen at the same moment, the remaining parts being concealed from view by the bars. Yet, since several parts of the same spoke are actually seen in a straight line through the successive apertures, it is not so easy to understand why they do not connect themselves in the imagination, as in other cases of broken lines, so as to convey the impression of a straight spoke. The idea at first suggests itself, that the portions of one spoke, thus seen separately, might possibly connect themselves with portions of the two adjoining spokes, and so on, forming by their union a curved image made up of parts from different successive spokes. But a little attention to the phænomena will show that

that such a solution cannot apply to them: for when the disc of the wheel, instead of being marked by a number of radiant lines, has only one radius marked upon it, it presents the appearance, when rolled behind the bars, of a number of radii, each having the curvature corresponding to its situation; their number being determined by that of the bars which intervene between the wheel and the eye. So that it is evident, that the several portions of one and the same line, seen through the intervals of the bars, form on the retina the images of so many different radii.

The true principle, then, on which this phænomenon depends, is the same as that to which is referable the illusion that occurs when a bright object is wheeled rapidly round in a circle, giving rise to the appearance of a line of light throughout the whole circumference; namely, that an impression made by a pencil of rays on the retina, if sufficiently vivid, will remain for a certain time after the cause has ceased. Many analogous facts have been observed with regard to the other senses, which, as they are well known, it is needless here to particularize.—*Phil. Trans.* 1825.

ENGRAVING ON ZINC.

There has lately been published by Leske the bookseller, at Darmstadt, the first large work of which the plates are executed in zinc. It is a collection of monuments of architecture, which will consist of twenty numbers. The work is done on zinc in the same manner as on stone, and the expense of engraving is thus avoided: hence the publisher has been enabled to sell the number, consisting of twelve folio plates, at five francs upon common paper. In an economical point of view, this method, therefore, deserves to be recommended. We see by the German journals, that M. Eberhard, author of the collection in question, has recently published a pamphlet upon the use of zinc, with the view of replacing copper-plates and lithographic stones, for engraving and designing. 8vo, with 10 plates. (Darmstadt, 1824.) *Edin. Phil. Journ.*

LIST OF NEW PATENTS.

To William Henry James, of Cobourg Place, Winson Green, near Birmingham, engineer, for improvements in apparatus for diving under water, and applicable to other purposes.—Dated 31st May 1825.—6 months to enrol specification.

To John Harvey Sadler, of Hoxton, Middlesex, machinist, for an improved power-loom for the weaving of silk, cotton, linen, wool, flax, and hemp, and mixtures thereof.—31st May.—6 months.

To Joseph Frederick Ledsam, merchant, and Benjamin Cook, brass-founder, both of Birmingham, for improvements in the production and purification of coal gas.—31st May.—6 months.

To

To Joseph Crowder, of New Radford, Nottinghamshire, lace-net manufacturer, for certain improvements on the pusher bobbin-net machine.—31st May.—6 months.

To Charles Powell, of Rockfield, Monmouthshire, gentleman, for an improved blowing-machine.—6 June.—6 months.

To Alfred Bernon, of Leicester-square, Middlesex, for improvements, communicated to him by a foreigner, in fulling-mills or machinery for fulling and washing woollen cloths, &c—7th June.—6 months.

To Joseph Apsdin, of Leeds, Yorkshire, bricklayer, for his method of making lime.—7th June.—2 months.

To Moses Poole, of the Patent-Office, Lincoln's Inn, Middlesex, gentleman, in consequence of a communication made to him by a certain foreigner, for the preparation of certain substances for making candles, including a wick peculiarly constructed for that purpose.—9th June.—6 months.

To John Burridge, of Nelson-square, Blackfriars Road, Surrey, merchant, for certain improvements in houses built of brick, or other materials, for the better ventilation of them, and other buildings.—9th June.—6 months.

To John Lindsay, of the Island of Herme, near Guernsey, esq. for certain improvements in the construction of horse and carriage ways of streets, turnpike, and other roads, and an improvement or addition to wheels to be used thereon.—14th June.—6 months.

To William Henry James, of Cobourg Place, Winson Green, near Birmingham, Warwickshire, engineer, for certain improvements in the construction of boilers for steam-engines.—14th June.—6 months.

To Jonathan Downton, of Blackwall, Middlesex, shipwright, for certain improvements in water-closets.—18th June.—6 months.

To William Mason, of Castle-street East, Oxford street, in the parish of St. Mary-le-bone, Middlesex, axletree-manufacturer, for certain improvements on axletrees.—18th June.—6 months.

To Charles Phillips, of Upnor, in the parish of Findsbury, Kent, esq., for a certain improvement or improvements in the construction of a ship's compass.—18th June.—6 months.

To George Atkins, of Drury-lane, Middlesex, gentleman, and Henry Marriott, of Fleet-street, London, ironmonger, for certain improvements on, and additions to, stoves or grates.—18th June.—6 months.

To Edward Jordan, of Norwich, engineer, for a new mode of obtaining power applicable to machinery.—18th June.—6 months.

To John Thompson, of Vincent-square, Westminter, and the London Steel-Works, Thames-Bank, Chelsea, and John Barr. of Halesowen, near Birmingham, engineer, for certain improvements in producing steam, applicable to steam-engines or other purposes.—21st June.—6 months.

To Thomas Worthington the younger, and John Mulliner, both of Manchester, Lancashire, small-ware manufacturers, for an improvement in the loom or machine used for the purpose of weaving tape and such other articles to which the loom may be applicable.—21st June.—6 months.

To Ross Corbett, of Glasgow, Scotland, merchant, for a new step or steps, to ascend and descend from carriages, &c.—21st June.—6 months.

To Philip Brooks, of Shelton in the Potteries, Staffordshire, engraver, for an improvement in the preparation of a certain composition, and the application thereof to the making of dies, moulds, or matrices, smooth surfaces, and various other useful articles.—21st June.—6 months.

To John Frederick Smith, of Dunston Hall, Chesterfield, Derbyshire, esq., for certain improvements in machinery for drawing, roving, spinning, and doubling cotton, wool, &c.—21st June.—6 months.

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr. BURNEY at Gosport, Mr. CARY in London, and Mr. VEALL at Boston.*

Days of Month, 1825.	Gosport, at half-past Eight o'Clock, A.M. Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Clouds. Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Height of Barometer, in Inches, &c. Lond. 1 P.M.	Bost. 8½ A.M.	Thermometer. London. 8 A.M.	Noon.	11 P.M.	Boston. 8¼ A.M.	Rain. London.	Boston.	Weather. London.	Boston.	Wind
May 26	29·70	62	49·90	58	N.	...	0·320	1	1	1	1	1	1	1	29·70	29·43	55	57	46	52	...	·67	Cloudy	Cloudy	E.
27	29·86	53	......	71	N.	...	......	1	1	1	...	1	1	...	29·81	29·45	46	55	45	45·5	0·10	...	Fair	Cloudy	W.
28	29·95	57	......	53	SE.	0·50	·220	1	1	1	...	1	1	1	29 87	29·60	47	54	44	50·5	...	...	Showery	Fine, rain *p.m.*	W.
29	29·95	56	......	56	NW.	...	·280	...	1	...	...	1	1	1	29·95	29·65	46	59	45	49	...	·21	Cloudy	Cloudy	W.
30	30·04	56	......	54	NE.	...	......	...	1	1	...	1	1	1	30·16	29·80	45	57	44	52	...	...	Cloudy	Fine	W.
○ 31	30·34	52	50·00	50	NE.	·40	......	...	...	1	...	1	1	...	30·40	30·10	44	55	45	50	...	...	Fair	Fine	W.
June 1	30·44	59	......	53	SW.	...	......	1	1	1	...	1	1	1	30·37	30·05	51	66	51	53	...	·16	Fair	Fine, rain *p.m.*	SW.
2	30·17	61	......	59	SW.	...	·065	...	1	1	...	1	1	1	30·04	29·70	56	65	56	57·5	·25	...	Cloudy	Cloudy	NW.
3	29·82	63	......	58	W.	·30	·320	1	1	1	...	1	1	1	29·78	29·40	57	63	52	53	...	·26	Showery	Cloudy, rain *a.m.*	W.
4	29·60	57	......	70	SW.	...	·270	1	1	...	...	1	1	1	29·36	29·16	54	57	50	55	...	...	Rain	Cloudy, rain *p.m.*	W.
5	29·58	54	......	53	NW.	...	·030	...	...	...	...	1	1	1	29·68	29·15	50	55	45	46	...	·26	Showery	Cloudy, and Stormy	SW.
6	30·04	55	50·05	50	W.	·40	......	1	1	1	...	1	1	...	30·02	29·65	48	64	54	53	...	...	Fair	Cloudy	W.
7	29·95	66	......	57	SW.	...	......	...	1	1	...	1	1	1	29·94	29·55	54	66	56	59	...	·12	Fair	Cloudy, rain *a.m.*	SW.
☾ 8	30·00	63	......	66	S.	...	......	1	1	1	...	1	...	1	30·01	29·60	56	69	57	60	...	...	Fair	Fine	S.
9	30·14	66	......	60	SW.	·60	......	1	...	1	...	1	...	...	30·20	29·63	55	68	58	63	·10	...	Fair	Fine	W.
10	30·35	63	......	54	S.	...	......	1	...	1	...	...	...	...	30·34	29·85	61	74	66	66·5	...	...	Fair	Fine	E.
11	30·30	66	......	53	NW.	...	......	...	...	...	...	1	...	...	30·26	29·77	64	75	66	70	...	...	Fair	Fine, Ther. 3 *p.m.* 80	SW.
12	30·23	68	......	52	N.	·90	......	...	...	1	...	1	...	...	30·19	29·70	67	79	65	72	...	...	Fair	Fine, Ther. 3 *p.m.* 80	S.
13	30·20	71	50·50	50	N.	·50	......	1	1	1	...	...	...	...	30·23	29·70	66	78	60	70	...	...	Fair	Fine, Ther. 3 *p.m.* 75	NE.
14	30·30	68	......	50	NE.	·60	......	...	...	1	...	...	...	...	30·35	29·90	62	76	60	61	...	...	Fair	Cloudy	E.
15	30·33	63	......	48	NE.	·40	......	1	...	...	...	...	...	...	30·33	29·93	60	73	59	64·5	...	...	Fair	Fine	E.
16	30·24	67	......	48	NE.	·40	......	...	...	1	...	1	...	...	30·20	29·80	60	76	58	65	·00	...	Fair	Fine	NE.
17	30·22	65	......	49	NE.	·40	......	...	...	1	...	1	1	...	30·23	29·85	55	68	49	59·5	...	...	Cloudy	Cloudy	E.
18	30·23	59	......	50	NE.	·30	......	1	...	1	...	1	...	...	30·23	29·85	54	66	55	61	...	...	Fair	Fine	NE.
19	30·13	62	50·60	50	SE.	·30	......	1	1	1	...	1	...	...	30·05	29·67	57	68	60	60	...	...	Cloudy	Cloudy	SW.
20	29·84	59	......	51	N.	·35	·390	...	...	...	...	1	1	1	29·80	29·40	55	55	49	60·5	...	...	Showery	Cloudy	W.
21	29·90	58	......	52	N.	·20	·220	...	1	1	...	...	1	1	29·93	29·55	50	57	47	54	...	...	Cloudy	Cloudy	W.
22	30·12	55	......	54	NE.	·25	......	1	1	1	...	1	...	...	30·10	29·72	52	65	55	58	...	...	Fair	Fine	W.
☽ 23	30·12	63	......	52	W.	·30	......	1	1	1	...	1	...	...	30·14	29·78	55	68	55	60	...	...	Fair	Fine	W.
24	30·07	60	......	49	SW.	·30	......	1	1	1	...	1	...	...	30·04	29·65	55	70	60	62	...	...	Fair	Fine	W.
25	29·88	67	50·80	49	SE.	·30	·020	1	1	1	...	1	1	1	29·79	29·40	62	73	55	64	·10	...	Showery	Fine	S.
Aver.:	30·066	61·09	50·31	54·1		7·70	2·135	18	19	25	1	26	17	14	30·05	29·66	55	66	54	58·2	0·55	1·68			

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PLATES.

ERRATA:

Page 159, line 2, *for* 1681, (under Nimbus,) *read* 1581.

Page 298, lines 16 and 19, *for* Rausselaer, *read* Rensselaer.

Vol. 64. page 211, line 5, *for* True altitude, &c., *read* True co-altitude, &c.

END OF THE SIXTY-FIFTH VOLUME.

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